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I. PHYSICAL CHANGES UPON THE SURFACE OF THE MOON.

By Edmund Neison, F.R.A.S.,
Author of "The Moon, and the Condition and Configuration of its Surface."

THE present condition of the surface of the moon is one of the most interesting and important questions within the whole range of Astronomy; and being a subject of considerable difficulty, and requiring a thorough acquaintance with more than one branch of science to be properly dealt with, it is not surprising that widely different views on the question are held by different astronomers. The question is, however, of importance to others besides astronomers, and many eminent physicists and geologists have recognised that the study of the present condition of the surface of our satellite promises to throw much light on many vexed geological and physical problems. The opinion of that eminent geologist the late Prof. Phillips on this subject is well known, for he often expressed his conviction that the lunar surface presented the best field for the study of many of the more difficult problems of geology, and under this conviction he devoted many years to the study of selenography. Considering, in fact, the close relationship between the earth and its satellite, and the general analogy that selenographers maintain exist between their respective classes of primitive formations, it is unquestionable that the study of lunar surface must throw light on many obscure points in the past history of the earth, and probably in fact on the general history of the planets.

Hist. of Science

The question of the present condition of the surface of the moon has engaged, therefore, the attention of some of the most eminent astronomers, and much has been done towards obtaining the information necessary for the proper consideration of the problem. But though much has been done in this direction, very much more remains to be done before this question can be held to admit of decisive settlement; for though the general nature of the lunar formations and surface has been made out fairly well, the details, which are of the utmost importance, are only very partially understood. The paucity of material available on which astronomers in general may found an opinion is not widely understood, for, with the exception of a few pages in the great work of Beer and Mädler's "Der Mond," and a small work by another great selenographer (Schmidt, of Athens), also entitled "Der Mond," until lately no published information may be said to exist accessible to astronomers in general. Hitherto information, with regard to the details of the structure of the formations on the surface of the moon, has had to be obtained directly by the careful study of the lunar surface,—a work necessarily occupying years, and only to be acquired by assiduous labour. These particulars, though not at first sight of importance, must not, however, be overlooked, for it will subsequently appear that they go far to explain the position now occupied by the question—What is the present condition of the lunar surface?

It is a remarkable circumstance, in relation to this question, that whereas those astronomers who have devoted much time and labour to the study of the moon's surface, and to whom astronomers in general are mainly indebted for our present knowledge of the surface of our satellite, hold in general one view as to the present condition of the lunar surface, astronomers as a body hold a very different opinion. To take a striking instance,—scarcely any astronomer known to have devoted time to the study of selenography doubts that many processes of actual lunar change are in progress, and it is doubtful if there is one who could not promptly instance one or more such cases. Yet the general opinion of astronomers appears to be against any such physical changes having occurred. And another instance, almost as striking, exists in connection with the subject of the lunar atmosphere: whilst all selenographers appear to have detected instances where the existence of this atmosphere is revealed, astronomers in general appear to question almost the possibility of its existence, and this in face of the absence of any evidence whatever that there is no atmosphere of the nature supposed.*

It would be an interesting inquiry to ascertain in what manner arose this direct conflict of opinion on this subject, between those who have systematically studied the appearances presented by the moon, and those who have not in the same systematic and assiduous manner examined the lunar surface. It appears to have originated in a short summary by Mädler of the appearances presented by the moon, wherein the differences between the condition of the moon and earth were forcibly stated, and where he pointed out the impossibility of the view that was held in the time of the earlier astronomers,—that the moon might be a mere copy of the earth, containing a dense atmosphere, large oceans, abundant vegetation, and animal life, or even human inhabitants. In a condensed and more unqualified form these remarks crept into all astronomical text-books, and were the main basis of the views commonly held by astronomers.

The present condition of this question is therefore not surprising; with the views of those astronomers who have devoted many years to the study of the moon, but very imperfectly known, and in fact inaccessible to most astronomers, the general body of observers are left to draw their own conclusions from their own acquaintance with the lunar surface. Approaching the study of the moon with strongly preconceived notions that the surface is a mere arid desert, nearly red-hot at times and almost immeasurably cold at others, without water, air, or life, the real condition of the moon is not such as to dissipate these views at the first glance. Its cold, still, apparently unchangeable surface, so utterly unlike what the earth might be supposed to appear as seen from the moon, convinces the casual observer that the world he then sees is utterly unlike the world he knows. He looks for immense cloud-masses floating in a dense atmosphere, and sees none; for wave-tossed seas and winding broad rivers, and there are none; searches for luxuriant forests and green prairies, and they are absent. This is enough, and he retires from further contemplation of the "airless, waterless, lifeless, volcanic desert" of the text-books. During subsequent periods he may again take a casual glance of our satellite, look at lunar occultations, or watch the grand changes in appearance that the face of the moon presents as the sun sweeps across its heavens and slowly illuminates in turn its varied formations. But study the surface, endeavour to piece together bit by bit the different features that are revealed, until he thoroughly comprehends the details and nature of the formations he sees, is what the general observer and the great majority of astronomers do not even attempt to do.* To properly become acquainted with the nature of the lunar surface requires that the study of the moon should be made the primary object of one's observations; and those who have done this, and devoted years to the assiduous study of the moon, constitute those astronomers who—as previously mentioned—hold different views as to the present condition of the lunar surface to astronomers in general.

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* The subject of the phenomena which would be presented by the moon were it surrounded by an atmosphere of any given density is one of very considerable difficulty. It cannot be treated in a popular—and therefore necessarily superficial—manner, because it involves subjects demanding a tolerably extensive acquaintance with mathematical physics to enable their real bearing to be understood. The mathematical treatment of the subject, though involving no point of any particular difficulty, is nevertheless complex and very laborious, from its extent. It is not surprising, therefore, that when this subject is regarded in a popular manner ideas should be arrived at that a more elaborate and careful examination shows, by mathematical demonstration, to be mistaken; and it was in this manner that Bessel, in his investigation of a portion of this subject, arrived at results which—though they have been since shown to be wrong—have exerted the greatest influence on the progress of selenography. In saying that there is no real evidence whatever that the moon does not possess the atmosphere which has been ascribed to it by the author, it must not be supposed that no attention has been paid to the various circumstances which have from time to time been pointed out as proving the impossibility of the moon possessing an atmosphere of a surface density far less than this. But on submitting these objections to the careful examination that they demand, and in particular by calculating with the aid of the powers of mathematical analyses what would be the real dimensions of these phenomena which it is supposed would reveal the existence of the lunar atmosphere, it is found that, for the atmosphere supposed, they would be insensible. Most of these objections have been already considered in my recent work on the Moon, or in the "Quarterly Journal of Science" (October, 1874). The whole subject involves such difficulties that statements founded on merely a popular or general consideration of the problem it involves require therefore to be regarded with caution.

The previous observations are, it will be seen, of importance in connection with the subject of the present article, for they show how selenographers and astronomers in general may hold very different opinions on questions connected with the moon,—one are led to conclusions from the careful study of the minuter details of the lunar formations, that the others, not thus acquainted with these features, refuse to accept. The purport of the present paper is to detail instances of what selenographers consider to be real physical changes in the moon, together with some of the reasons which they have for supposing them to be such. From an opposite point of view the subject has been already ably handled in these pages by one of our most eminent astronomers, Mr. R. A. Proctor, who in his own department of the science stands still unrivalled.*

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* An interesting instance, illustrating the condition of the general knowledge of selenographical subjects amongst astronomers, is the prevailing opinion that no retardation of occultation of stars by the moon has ever been observed, but that stars approach the limb of the moon, and disappear behind it at the instant they theoretically should. Yet it has been stated repeatedly that the only three series of reductions of the observation of occultation of stars show that a very considerable retardation does take place—a retardation of from 5 to 10, and often even 15 seconds of time. Different opinions may be held as to the cause of this retardation of occultation, but of the fact there can be no question, and it has been known for nearly twelve years.

The most prominent instance of a supposed change on the surface of the moon is indubitably that of Linné, because it is an instance which was brought before the entire astronomical world, and engaged for nearly a year the attention of almost every astronomer in Europe. The leading facts of the case may be briefly stated as follows:—On the north-west quadrant of the moon is a large tract of nearly level surface, of a pale grey, almost greenish colour, and this region—which is in extent about 430 miles in diameter—contains near the centre a moderate-sized bright crater, called Bessel, nearly 14 miles in diameter, and with a circular wall rising 4000 feet above the interior and about 1600 feet above the surrounding surface. Scattered about this plain are a few small crater-like objects, about 2½ miles in diameter and with walls rising about 300 feet high. Now, near the eastern centre of this great tract of level ground, or the *Mare Serenitatis* as it is called, one of the most eminent selenographers, Lohrmann, had placed a distinct bright crater, subsequently called Linné, and this crater he described as being about 5 miles in diameter, and, after Bessel, the most conspicuous object on the Mare Serenitatis. Ten years later our greatest selenographer, Baron von Mädler, confirmed Lohrmann's description, and named the crater Linné, which he described as deep and very distinct in oblique illumination. He measured its diameter, which was 6½ miles, or about half the size of the great crater Bessel, and selected it as the most distinct object in its region, for one of the measured units in his great trigonometrical lunar survey. Now, had Lohrmann's account not been correct, Mädler would certainly have been struck at once by the fact; but Mädler's work, which was entirely independent of his predecessor, entirely confirms the account. And had Mädler's account been incorrect, the instance would have been so evident that it could not have escaped the attention of Gruithuisen and Schmidt, who were about this time making lunar observations, and especially the latter, as he repeatedly drew this particular part of the moon. In the drawings of Schmidt, however, whenever this crater Linné is shown, it is as a deep crater, entirely in accordance with the description of Lohrmann and Mädler.

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* Quarterly Journal of Science, January, 1873, and October, 1873.

In October, 1866, however, Schmidt was startled by finding no trace of Linné, when it was in a position when it should have been most conspicuous. Instead of the deep wide crater with its interior filled with intense black shadow, all he could detect was a faint, indistinct, cloudy marking, about 5 miles in diameter. During the subsequent evenings it retained this aspect, though at times it was so indistinct as to be barely visible. Schmidt immediately announced this startling circumstance, and during the end of 1866 and the commencement of 1867 nearly every astronomer in Europe directed his attention to this now-celebrated spot. Since then this portion of the moon has been repeatedly examined by selenographers, but Linné has never been seen of the size and character ascribed to it by Lohrmann, Mädler, and Schmidt. The conspicuous deep crater has utterly disappeared.

In what manner is this change to be explained, and how is it that apparently so unmistakable an instance of physical change has been rejected as not established? These questions are best answered by describing what has been seen in the place formerly occupied by the deep crater. Where Linné was, it has been already mentioned, an indistinct white cloud-like mark was seen; but powerful telescopes and assiduous examination soon revealed something else. When first the sun illuminated the region containing Linné a small conical mountain peak was detected, this mountain being about 100 to 200 feet high, and casting a short black shadow, and was by some mistaken for a small crater. Subsequently other observers detected a minute crater cone. When first seen it was estimated by Schmidt to be about a quarter of a mile in diameter, and by Secchi, of Rome, and Buckingham as about one-third of a mile across; this crater lying to the west of the small hill. If the estimates of these earlier observers are to be trusted, and Schmidt at least was not likely to be mistaken, a most important circumstance must have occurred; for when next seen the crater was nearly three times as large, or nearly 1 mile in diameter, according to D'Arrest of Copenhagen, Schjellerup, Huggins, and Wolf. In July, 1867, the crater was sufficiently large and distinct to permit of its being measured, and according to Huggins it was rather under 2 miles in diameter—a result confirmed, during the next month, by Buckingham and Knott. It must be remembered, however, that this crater was only seen when the region was very obliquely illuminated by the sun, at sunrise or sunset in fact. At any other period all that was seen was a white cloudy marking, about 8 miles in diameter. Since 1868, it is only on three or four occasions, when the atmosphere has been exceptionally good, that the small crater within Linné has been seen, and then only in powerful telescopes. It does not seem ever to have been seen as a distinct crater in telescopes as small as those employed by the earlier selenographers, who only knew the crater as a fine, distinct, conspicuous object, with a black interior, nearly five times the diameter of that of the present crater. The present diameter of the crater opening of the cone on the site of Linné is about 1¼ miles, the total diameter of the cone at the summit about half as much again, and at the base about 2½ miles. The height above the surface is about 200 feet at most.

As a crater, in fact, Linné is now one of the most difficult to see on the entire Mare Serenitatis. Under illumination, when every other crater stands out boldly as distinct objects, Linné is, with rare exception, either invisible as an elevation or else appears as an insignificant hill. It is questionable whether the earlier selenographers could have seen it at all under these conditions. It should be mentioned, however, that between 1867 and 1869 several observers considered that they could detect traces of the ring of a very shallow crater around the crater cone in the position of Linné, and in size slightly larger than the crater of Beer and Mädler. The actual existence of this ring is doubtful; it has not been seen since 1869, and the appearance noticed probably arose from a number of ridges and mounds near the position of Linné.

Considering, then, the facts as stated above, it would appear that there could be no question but that a real physical change had occurred on this portion of the surface of the moon. The large crater independently described by Lohrmann, Beer and Mädler, and Schmidt, as existing in this portion of the Mare Serenitatis, unquestionably no longer exists, and in its place appears a white cloudy marking, containing a small crater cone, with an opening scarcely one-twentieth of the area of the former crater. What reasons have therefore been advanced against any change having taken place, that astronomers in general should favour this view? The principal reason why astronomers are so strongly against allowing that any change can have occurred appears to be the existence of a strong prejudice against the very possibility of such changes occurring. The probable origin of this feeling has been already adverted to. This of itself, however, would not have been sufficient, of course, to bring about this result; it remains, therefore, to consider the evidence against the reality of any change in Linné having occurred that has from time to time been brought forward.

Immediately after Schmidt had announced the supposed change in Linné, reference was made to the lunar drawings in the "Selenotopographische Fragmente" of Schröter, of Lilienthal, the earliest of the great selenographers. Amongst the very earliest drawings was found one of the Mare Serenitatis, made on November 5th, 1788, with powers of 95 and 161, on a reflector of 6 inches aperture and 7 feet focus. On this drawing Schröter does not draw Linné as a crater, but not very far from its place he draws a white spot on a ridge which he marks *v*, and a larger dark spot which he marks as *g*. Schmidt considered this white spot *v* to be Linné, and this view has been strongly urged by Huggins, and has been generally accepted as correct by astronomers. It has been urged, therefore, that as Schröter drew this formation in a manner not unlike its present appearance no change can have occurred, but that Lohrmann, Beer and Mädler, and Schmidt must all have been entirely mistaken as to what they thought they saw. It was also found that, on reference to a map made by Lahire during the seventeenth century, no trace of Linné is to be found. It may be, however, at once observed that this last fact is absolutely valueless; the map of Lahire is perfectly untrustworthy in these minute details, and Hevelius, Riccioli, and Cassini are little superior. From all the maps made about this date numerous craters are omitted, far larger than Linné ever was supposed to be, so that the fact of Linné not having been drawn proves nothing. These maps were made principally by full-moon drawings, where Linné would not be visible as a crater. Even, however, had this not been so, the value of this negative evidence from Lahire's map is entirely destroyed by the direct evidence furnished by Riccioli's map, which shows Linné as a distinct crater; and it must be remembered that the present crater on the site of Linné could not possibly have been seen by Riccioli with the optical means by which the materials for his map were obtained.

The only real evidence, then, that no change has taken place in Linné is this single drawing of Schröter's, which is relied on as proving that the subsequent maps and drawings of Lohrmann, Beer and Mädler, and Schmidt must be entirely wrong in this point. It is a remarkable circumstance that in every other case of a discrepancy between the drawings of Schröter and Beer and Mädler, one alone of the above authorities, the drawings of Schröter, have been rejected as entirely unworthy of comparison with those of Beer and Mädler. In this particular instance, however, one of the earliest drawings of Schröter, made with his most imperfect and least powerful instruments, on an occasion when the definition from his own account must have been very inferior, has been brought forward to prove the incorrectness of the drawings and micrometric measures of his great successors. Now, no selenographer acquainted with Schröter's works will allow that this is permissible. Schröter's early drawings are never to be trusted when they differ from his successors in the more minute features. Whenever a discrepancy exists, it will be found that Lohrmann or Beer and Mädler are correct, and Schröter wrong. It is true the same does not hold with Schröter's later drawings, and especially when he was in possession of his great reflectors of 13 feet and 26 feet focus respectively; but this particular drawing of Schröter's is one of his very first. It must also be remembered that Schröter did not draw the portion containing Linné with the same fulness or accuracy as the rest, but that, while the western portion is fairly correctly drawn, the eastern portion, where Linné should be, is misplaced and imperfect. Mr. Birt, our best English selenographer, doubts in fact whether this white spot, *v*, really is meant for Linné, and has made out a very strong case for supposing that the dark grey spot, *g*, farther south, really represents Linné as seen by Schröter. If this is really the case it entirely upsets the view commonly held by astronomers that Schröter's drawing proves no change to have occurred in Linné. For Schröter describes this spot *g*, which was only faintly illuminated, from its being near the dark portion of the moon, as being very similar to, and probably of the same nature as, another smaller formation a little to the west of it. Now this smaller formation, *r*, is really a fine distinct crater, Bessel *m*, about 3 to 3½ miles in diameter and 600 or 700 feet in depth. If, then, the spot *g* is Linné, as seen by Schröter, it would have been a large crater, in tolerable accordance with the description of Lohrmann and Beer and Mädler. Further, as Mr. Birt points out in relation to the western and best shown portion of the drawing of Schröter, *g* falls exactly into the position which Linné should occupy.

For the reasons detailed in the above, therefore, selenographers consider that the attempt to show that Linné has not changed has entirely failed, but that in the case of this crater we have an instance of real physical change on the moon. With regard to the nature of this change little definite has as yet been ascertained, and it will require a long examination of this region with powerful telescopes to determine what change really has occurred. From numerous observations the explanation agreeing best with the present conditions of the surface is that the walls of the old crater have collapsed and fallen into the interior. By this means the interior would be nearly completely filled up, leaving, however, a sort of rough, cone-like, small crater towards the centre. Under exceptionally favourable atmospheric conditions, with the assistance of a powerful telescope,* the surface immediately around the small crater has been seen to present the appearance of being rough and irregular. Round what would have been the border of the old crater are numerous mounds and rough blocks, and on the east is one if not two low hills or peaks, presenting the appearance of being portions of the old wall. The difficulty of making these observations is very great, and they are only possible in the very finest atmospheric conditions. A prolonged examination of this region, however, would amply repay the labour.

There is one other point on which a remark must be made, and that is with reference to an apparently generally spread misconception of the change which Linné is supposed to have undergone. Thus, in the interesting article on this subject in the "Quarterly Journal of Science" for October, 1873, Mr. R. A. Proctor says:—"Mr. Browning, after considering the evidence afforded by his own observations, considered in connection with those made by others, arrives at the conclusion that 'there is scarcely any ground for supposing that any change has occurred in this small but celebrated crater.'" Mr. Proctor then adds—"These remarks appear to me to contain the gist of the whole matter. We see that Linné has a surface so constituted that as the sun is rising there, and so pouring his rays very obliquely, there is a continual change of aspect precisely resembling that which can be recognised when certain kinds of rock surfaces, and especially crystalline formations, are viewed under oblique illumination. We know that in such cases the tints vary, not only absolutely but relatively, insomuch that a part which is darker than another with one oblique illumination will be lighter under another and but slightly different illumination.

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* With an aperture of 9⅓ inches, and a power of 600.

"It appears to me that no other explanation can reasonably be suggested, because, in point of fact, we have to choose between the theory that there has been a definite change of surface on this part of the moon, or that the change is only apparent. Now, if there has been a definite change at any time, fresh changes must have restored, either from time to time or definitely, the former condition of the surface. But this seems extremely unlikely, whilst such a change as Sir John Herschel considered to afford the best explanation of Schmidt's observation may be regarded as one which no subsequent process could so modify as to restore, or nearly restore, the original appearance of the region." (This explanation of Sir John Herschel's referred to was that the crater of Linné had been filled up to overflow with viscous lava.) "Such a change would doubtlessly account well for the observed appearances, but it leaves the subsequent restoration of the crater unexplained."—(Pp. 500, 501.)

From the above quotation Mr. R. A. Proctor would appear to consider that the entire known observations that have been made of Linné can be explained by supposing the surface to vary in tint with differences of illumination, and he also seems to suppose that of late years Linné has been seen to present the same appearance and possess the same characters as it was described as having before the supposed change occurred. But it has been already shown that this is entirely a misconception: whatever change occurred in Linné was definite, and since 1867 Linné has never been seen to accord with the description of the early selenographers. To suppose the minute crater-like formation now existing on the site of Linné can be in any manner identical with, or even similar to, the twenty times larger formation that was observed, drawn, and measured by Lohrmann, and Beer and Mädler, is out of the question. Nor will any selenographer allow that differences of illumination, of any kind or on any surface, are capable of explaining the difference in appearance of the formation at the two different epochs. No alteration in illumination whatever could make an object where Linné is placed look at one period like a considerable and deep crater, and at another as a small, scarcely visible crater. The means by which Mr. R. A. Proctor would explain the supposed change in Linné are perfectly inadequate. Nor will the effect of variation in illumination even account for the minor variations which it has been supposed have occurred since 1866, such as, for example, the alteration in the size of the small crater within Linné from a diameter of barely one-third of a mile in January and February, 1867, to a diameter of five times as great in July, 1867. It is true the reality of many of these supposed changes is doubtful, but alterations in the angle of illumination are inadequate to explain them *per se*. In fact, so far as the known observations extend, the alterations undergone by Linné in its present condition appear to be perfectly normal, and exactly similar to the changes undergone by all other analogously placed and constituted formations. The changes are striking, but in no manner strange or unexpected to the experienced selenographer.

The facts about Linné may be therefore summed up very briefly. According to three or more independent selenographers, the most experienced and eminent that Science has seen, the object named Linné was a conspicuous crater of large diameter and great depth. Now in its place all that exists is a tract of uneven ground, containing a small, scarcely visible, insignificant, crater-like object. It is impossible that one could ever be systematically taken for the other. It is inconceivable how our three greatest selenographers could have systematically and independently made the same blunder, and that one blunder only. For in no other case do we find any error of this nature. Their description must therefore be held to truly describe the nature of the formation at their epoch (1820—1845). The object is no longer of the same size and description. A real physical change on the moon's surface must therefore have occurred at this point. This, then, is the conclusion that selenographers as a body have arrived at, yet, despite the strong evidence on which it rests, it is not generally recognised by astronomers.

The next instance of supposed physical change on the surface of the moon is one of the most peculiar in the entire range of astronomical observations, and is that in the case of the crater named Messier by Beer and Mädler. In the equatorial region of the moon, on the *Mare Fæcunditatis*, the westernmost of the great lunar plains, close to one another are two small crater plains, according to Beer and Mädler about 9 miles in diameter. These two formations lie isolated on the open plain, and are surrounded by only some very low ridges, and mounds, and some rounded depressions like craters. From the easternmost of the two, Messier A, extend two long, slightly diverging streaks, of a pale grey colour, which in full moon give the formation much the appearance of a bright comet with a long double tail. Schröter, of Lilienthal, discovered this object, and drew it carefully, making the western formation of the two —or Messier itself—slightly the larger. He also suspected that its appearance was variable. In consequence of the observation of Schröter, Beer and Mädler paid particular attention to these formations, examining them most carefully on more than three hundred distinct occasions, between 1829 and 1837. They were thus enabled to declare with certainty that the two formations, Messier and Messier A, were exactly alike in every manner, not the slightest difference in any particulars being detectable. Both were circular crater plains, 9 miles in diameter, with 7° bright greyish white walls, surrounding a yellowish grey interior only 3° bright. On the walls, which were of the same height, were several wall peaks, in both formations situated in the same position with regard to the formation. In fact, as Beer and Mädler draw particular attention to, their own observations are decisive proof that the two formations were exactly equal in every respect, and that in diameter, form, height of their walls above the surrounding surface, and depth of the interior beneath the crest of the walls, colour of the interior and of the walls, and the position of the surrounding wall peaks, Messier and its twin formation Messier A were completely alike. And it is impossible that any difference in these respects would have escaped the attention of the greatest selenographer of our day, during a course of over three hundred observations in a space of nine years.

Some years after this, Gruithuisen—a most zealous and keen-sighted, though fanciful, observer—detected a slight dissimilarity between the form of the two craters, but this fact received little, if any, attention. In November, 1855, the Rev. T. W. Webb, one of the best living lunar observers. who was examining this region with a telescope of similar dimensions to that of Beer and Mädler's, observed that the eastern crater plain appeared the larger of the two. On again looking at the formations on March 11th, 1856, he at once detected that not only was the western crater plain, or Messier the smaller of the two, but that it was elliptical, with its greatest diameter extending from east to west. This fact was confirmed by subsequent observations and from drawings made by him in 1857, whilst Messier A, the eastern formation, appears to have remained unchanged, still being a circular crater plain, with a diameter of 9 miles, the western crater plain, Messier itself, had an elliptical form, with a long diameter of about 10 to 11 miles and a short diameter of about 7½ to 8 miles. The most rigid equality between the form and dimensions of these two formations, so strongly asserted to exist by Beer and Mädler, then no longer existed in 1857, according to the observations of the Rev. T. W. Webb.

This fact was of very great selenographical importance, and should have attracted general attention, because whatever uncertainty may be attached to the observations of Linné made by Beer and Mädler, it cannot be seriously urged that Beer and Mädler could have failed to recognise the dissimilarity between Messier and Messier A discovered by the Rev. T. W. Webb, when we consider that for over three hundred times they most carefully examined the two formations for the express purpose of detecting any difference. Nor can that potent agent, the lunar librations, be invoked to account for this difference, for in this particular instance it is entirely without sensible influence. For Messier lying very close to the moon's equator, and in longitude 47° W., no variation in the libration of the moon can sensibly affect the meridianal apparent diameter of the formation, and it is in this last that the diminution has been observed. If it was established, therefore, that the two formations were no longer alike, but differed markedly in any respect, the very strongest evidence of actual physical change would be the consequence.

Although therefore it is thus important that further observations of these formations should be obtained, the matter attracted little attention until the period 1870 to 1875. During these years Messier and Messier A have been repeatedly examined with the aid of most powerful telescopes, and the present dissimilarity between Messier and Messier A has been placed beyond the possibility of doubt. From some measures during the past year the long diameter of Messier appears to be 12·2 miles, and the short diameter—which is nearly, though not quite meridianal—is 6·9 miles. The difference between the form and dimensions of the two formations is now obvious in the smallest astronomical telescope, for it is unmistakable with a power of 150 on a telescope with an aperture of a little over 2 inches. With the fine Frauenhofer equatorial of Beer and Mädler, with an aperture of nearly 4 inches and a power of nearly 300, the difference now existing between the two formations would have instantly arrested the attention of any selenographer. It is inconceivable that the two formations could have possessed their present form in Beer and Mädler's time, and have been on hundreds of different occasions carefully scrutinised to detect any difference that might exist between them, without the most marked dissimilarity which now exists being detected.

The most difficult portion of the question remains, however, still untouched ; for even were it granted that in the case of Messier there exists an instance of real physical change, the process by which that change could have been brought about seems to defy explanation. In the case of Linné we have a plausible and easy method of accounting for the observed phenomenon, but in Messier what it seems must have occurred is a slow squeezing of an immense crater plain out of shape. The nature of the forces and processes at work upon the surface of the moon, which could compress a circular crater 9 miles in diameter into an elliptical formation some 12 miles by 7, is such with which there at present exists nothing analogous on the surface of the earth ; and until some satisfactory theory could be devised which would show by what means this change in Messier could have been brought about, in a manner consistent with other selenographical facts, it is not surprising that a strong reluctance should exist to admitting that a change had occurred in the form and dimensions of Messier, even when backed by the extremely strong evidence in its favour that has been adduced.

It is possible, however, that this difficulty may be avoided, for careful examination of the crater plain Messier and its neighbourhood suggests that, instead of a bodily compression of the entire crater, the observed change may have occurred from the gradual sliding of the north and south walls into the interior, and in so doing pushing the entire western wall, outwards and westwards, down an incline existing there. If this explanation is found to be consistent with the present condition of the formations, it at once removes all the difficulties, because the process it involves—namely, the gradual slipping of the wall of a formation into the interior—is one of which numerous traces exist upon the moon. There would be little difficulty on the part of selenographers in pointing to a hundred instances where a like circumstance had occurred,—one within Gassendi being a notable case, and one where a massive wall has been pushed back nearly 10 miles. As far as is at present known, this explanation is in accordance with the condition of the surface immediately around Messier, but further observations with powerful telescopes are indispensable. Unfortunately the region of the moon containing Messier is one which cannot often be observed at the proper time for this purpose ; so that only one or two days during the year are likely to be found when Messier is in a proper position to be observed, and the weather will permit a high power and a powerful telescope to be used to advantage, for these require very steady and favourable atmospheric conditions.

It is not, however, in these two instances alone that selenographers have detected instances of changes in the lunar surface ; but these two instances are cases where there exist the very strongest evidence that real physical changes have occurred. There are various other remarkable cases which render it very probable that different small portions of the moon's surface have undergone striking variations ; but though they rest on evidence that in other branches of astronomy have been usually accepted as sufficient to establish the existence of such variations, they do not rest on evidence of so overpowering a weight as is now alone considered worthy of credit on these selenographical questions. Thus towards the central portions of the moon, on the wide open plain called the *Mare Nubium*, according to Beer and Mädler, when they drew their map and whilst they were making their observations, there existed a fine, deep, glittering white crater, called by them *Alpetragius d*. It had a diameter of 5 miles, and close to it, on the south-west, was a smaller crater, about 1 mile in diameter. The large crater Alpetragius *d* has entirely disappeared, leaving in its place a slightly less bright, perfectly round, greyish white spot, with a diameter of 7·2 miles. The smaller crater, however, remains unaltered, and corresponds to Mädler's description. We have here, in fact, an exactly similar case to the well-known instance of Linné. About 240 miles south of this last region, on the border of the grey Mare Nubium, is a fine though ruined walled plain, named *Hesiodus*, and consisting of a level plain nearly 20 miles in diameter, surrounded by the ruins of a mountain border. Beer and Mädler describe and draw this formation as containing a perfectly level interior, grey in colour, and becoming slightly lighter towards the centre. Within it now, nearly in the centre, is a fine deep crater, 4 miles in diameter, and very distinct long after sunrise, so that it seems difficult to understand how it could have escaped Beer and Mädler's notice, had it then existed like it is now. Many other instances of a similar nature might be easily quoted.

Selenographers have also been long acquainted with variations of a different nature, occurring in lunar regions, and consisting in variation in colour or brightness. Beer and

Mädler, who were the first to notice the existence of definite variations in tint, were struck by the analogy they bore to the changes which would result from processes of vegetation on the lunar surface, though, as they pointed out, this seemed incompatible with the absence of masses of water upon the moon. It is remarkable that, despite this apparently fatal objection, other experienced selenographers have been led to the same conclusion, from evidence of widely diverse nature. Other instances of colour changes on the moon, also periodical in character, appear to indicate the effects of the solar rays continuously poured down on the lunar surface for nearly fifteen days at a time; and to this class appears to belong the variation in the floor of Plato, which has been discussed by Mr. Proctor in the "Quarterly Journal of Science" for January and October, 1873. There are other instances which, unlike the last, are not periodical in nature, and which indicate in a very forcible manner the effect of the slow, but energetic and increasing, disintegrative agents which have destroyed all the older lunar formations, and the principal of which appears to be the extensive and all-powerful lunar atmosphere. The capabilities and power of this last member of the forces moulding the surface of the moon are generally overlooked, on the supposition that its small density, when compared with the extremely dense atmosphere of the earth, must destroy its energy and capabilities, forgetting that what it may want in density it may make up in volume.*

The variation in the tint of the floor of Plato is one of the most interesting of these changes in tint, because its reality and its periodical character have been established in an indisputable manner. Plato is a circular plain, 60 miles in diameter, lying depressed beneath the surface of a great belt of highlands, which separate two of the greater lunar dark plains from one another. The formation lying in 50° N. latitude is seen from the earth in perspective, and appears to be foreshortened into a marked ellipse. It is bordered by a moderately steep slope, from 3000 to 3500 feet in height, broken here and there on the west and east by peaks nearly as high again. On the south it is separated by this highland border from the dark plain called the *Mare Imbrium*, to the level of which the border falls in a gentle slope, the distance between the floor of Plato and the Mare Imbrium being about 10 miles. The plain forming the interior of Plato is almost level, being broken only by a few very small steep crater cones. The colour of the highlands around Plato is a pale yellowish grey at sunrise, gradually becoming a greyish white as they are more intensely lit up by the solar rays. The grey plain on the north of Plato, called the Mare Imbrium, is a pure yellow-grey at sunrise, gradually increasing in brightness until a light yellowish grey at full moon. These details are necessary to properly understand the change which occurs in the tint of the floor of Plato as the lunar day progresses from sunrise to noon, and thence to sunset. The details of the changes experienced are founded on six years' observations.

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* It is to be remembered that the influence of the atmosphere upon the surface of the moon depends almost entirely upon its mass, and not upon its surface density. It is not through its pressure that the lunar atmosphere exerts its influence upon the surface of the moon, but through its chemical action upon the constituents of the crust of the moon, or through modifying the great variations in temperature to which the lunar surface is exposed. In its chemical action the effects of the atmosphere will depend almost entirely upon its mass, because, through diffusion and similarly acting causes, all portions of the atmosphere will successively come in contact with the surface. Were the action of the atmosphere very rapid, so that the entire chemical power of any portion was liable to be exhausted before its place could be taken by another portion through the agency of diffusion, then it is true a decrease in the density of the atmosphere might produce a decreased action in a given time. But the action of the atmosphere is so excessively slow that any effect of the kind stated is out of the question. The effect of the atmosphere in modifying the variation in temperature to which the lunar surface is exposed likewise depends almost entirely upon the mass of the atmosphere, and not upon its surface density. Thus, in the retardation of the radiation of heat from the moon, and in the absorption of the solar heat, it is not the surface density of the atmosphere on which these effects depend, but, as both Tyndall and Magnus have shown, upon its mass. Yet the strange idea that the powers of the lunar atmosphere lay entirely in its surface density seems to be entertained by a hostile reviewer of my work on the Moon.

At sunrise the interior of Plato appears a pure cold grey in colour, whilst the surrounding highlands are a yellowish grey, and the neighbouring Mare Imbrium a cold yellow-grey, on the western portion of the floor being the deep black shadow of the border, and on the eastern side the greyish white illuminated slope of a lofty peak. As the sun rises higher above the horizon of Plato, and the solar rays fall more perpendicularly on this region, the whole surface grows rapidly brighter, until, about two days after sunrise, the interior of the formation attains its brightest tint. It is then a cold light yellow-grey, often approaching a pale yellow in fact, and brighter than the surface of the Mare Imbrium on the north, whilst the surrounding highlands are a bright greyish white, tinted here and there with grey. Judging from what occurs in any of the numerous other formations resembling Plato, this may be considered the normal tint, inasmuch as those other formations which present exactly the same phenomena up to this time, and which under similar conditions present exactly the same appearance, retain this tint unaltered until near sunset. After, however, the second day, the floor of Plato commences to undergo a most extraordinary and anomalous change, which renders it unique on the moon, for instead of growing lighter the interior commences to become darker. Four days after sunrise it is materially darker than the northern Mare, and a cold grey in tint, whilst the surrounding highlands are a bright white in colour, tinted with grey; the appearance they retain until the thirteenth day after sunrise growing a little, though not very much, brighter towards full moon. Two days later the floor of Plato has become a dark grey; at full moon it is deep steel-grey; and about two days after full reaches its darkest tint, a very deep steel-grey, almost approaching a black colour. Under these conditions it is one of the very darkest portions of the entire lunar surface, though seven days prior it was one of the lightest portions of the surface of its kind. After this it gradually lightens in tint, but much slower, and never reaches so light a tint.

This extraordinary periodical change in the tint of the floor of Plato has hitherto received no explanation, but its existence has been put beyond the pale of doubt, and Mr. Birt has, at the instance of a British Association Committee, carefully discussed a numerous series of observations, made in the years 1869, 1870, and 1871, by six or seven independent observers. The character of this change, however, has yet to be examined to ascertain whether it is due to a real alteration in the tint of the surface, or is merely an optical variation due to either the physiological effect of contrast or to a peculiar conformation of the surface, or to some cause of similar nature.

It is not impossible to imagine a peculiar conformation of the surface which would cause it to undergo changes somewhat resembling those observed in the case of Plato, and in fact many such analogous explanations might be devised. It, however, must be borne in mind that, though this is possible, it involves what is extremely improbable, because none of these peculiar conformations can be considered in any manner likely. Thus, by supposing the surface of Plato to be covered by a number of conical elevations, each of a particular shape and height, and distributed in a particular manner, and with a systematic relation to every other such conical hill, the broad features of the phenomenon might be accounted for. This is invoking, however, the aid of a most fanciful arrangement, and the probability of any such explanation is too small to be seriously urged for consideration.

Mr. Proctor has advanced, however, a very ingeniously-contrived hypothesis to account for the phenomena observed, and which he regards as being probably the effects of contrast, and this view he has backed by some sound arguments. Mr. Proctor would seek the explanation of the variation in the floor of Plato in the same source as that of the well-known phenomena of a dark body looking still darker when on a brighter background. Thus at sunrise the floor of Plato is thrown against a dark background, due to the sombre, barely illuminated, surrounding regions, whilst at full moon it has for a background the brilliantly illuminated surrounding highlands, and should look much darker. Thus ("Quarterly Journal of Science," vol. xl., p. 504, Oct., 1873) Mr. Proctor says—"Setting aside other possible explanations . . . there is the effect of contrast to be considered. This I believe, from my own observations, to afford the true explanation of the observed phenomena. Plato lies on lunar highlands, which shine very brilliantly under high solar illumination. Towards the Mare Imbrium a comparatively narrow ridge separates the floor from that region. Now when the terminator has just passed beyond Plato, the surrounding wall is not nearly so bright as at the time of full moon; the black shadow of its western ridge occupies the western side of the floor; and the eye, in estimating the tint of Plato, is neither oppressed with the glare of general light on the one hand, nor forced to compare the tint of the floor directly and solely with a much brighter surface; if the comparison is made on the east, with an illuminated wall, it is made on the west with a perfectly black shadow-streak. Similar remarks apply to the time when the terminator is about to pass away from Plato. But at the time of full moon the highlands around Plato are very brilliantly illuminated. The glare necessarily makes Plato itself look relatively dark, notwithstanding the fact that the floor is also much more brilliantly illuminated; for it is a recognised fact, that surfaces of unequal light-reflecting capacity appear to differ more in brightness under a high illumination than when they are only faintly illuminated. We know, in fact, that a surface which is only dark looks almost or perfectly black when itself and a brighter background are under strong illumination." Mr. Proctor proceeds to illustrate this by reference to the fact that occasionally some of Jupiter's satellites look bright when seen against the sky, and yet dark when seen against the bright surface of the planet when in transit, and then continues—"Such an observation as this appears to me to be decisive against mere eye-estimations, showing that absolutely no reliance can be placed on them unless some contrivance is employed to destroy the effect of contrast. As it is conceded that none of the observed darkenings of the floor of Plato have been other than eye-estimations, no further discussion seems needed, or can in my opinion be legitimately given to the subject. I may mention, however, that having, though as yet in an imperfect manner, studied Plato with a much reduced field, so that I could eliminate to some degree the effects of contrast, I have not found that the floor grows relatively darker towards the time of full moon."

This most ingenious explanation devised by Mr. Proctor to account for the phenomena presented by the floor of Plato, and by assuming the correctness of which he is enabled to draw the very strong conclusions just quoted, is not an explanation which will be for one moment admitted by astronomers who are familiar with the phenomena presented by the various lunar formations, for they at once perceive numerous and apparently fatal objections to it, when applied as Mr. Proctor proposes to do: its very nature, in fact, suggests the idea that its able author overlooked the relationship in which Plato and the anomalous darkening of its floor stood to other similar formations with the normal behaviour of their interiors, for his hypothesis supposes nothing exceptional in the constitution of the walled plain Plato. If, then, the great darkening observed to occur in the tint of the interior of Plato is merely apparent, and only what must occur when a darkish walled plain is surrounded by a bright background, or rather bright environs,—and this is all Mr. Proctor ascribes to it,—it must be a perfectly normal occurrence, and the same must take place in every similarly placed formation, unless that has something anomalous about it to prevent this taking place. But all selenographers could instance a number of such walled plains where no such darkening occurs. Are we, then, to assume that all these possess anomalously constituted interiors, and that only Plato, of all the lunar formations, exhibits the normal phenomena. This is, of course, entirely inadmissible, and selenographers are thoroughly aware that the effects of contrast alluded to by Mr. Proctor are entirely incapable of bringing about such an immense darkening in tint as is apparent in the case of the floor of Plato.

As an instance illustrating this, the behaviour of the walled plain named Archimedes may be considered in connection with that of Plato. This walled plain lies not very far to the south of Plato, and is, if slightly smaller, of the same form, and bordered by mountains of the same height and brightness; whilst lying on the open plain it is environed by a surface little inferior in brightness to that around Plato. At the period when the anomalous darkening of the floor of Plato commences, the interiors of the two formations are sensibly of the same tint, a cold light yellow grey, both being brighter than the surface of the intervening Mare Imbrium. But while the interior of Plato darkens until it is almost black, the floor of the similar formation Archimedes grows gradually lighter until it is a bright yellowish grey, thus following the same course as nearly every other lunar formation. If, then, the darkening presented by the floor of Plato is, as Mr. Proctor contends, the natural result of the contrast between its surface and its bright environs, why does not Archimedes, similarly placed, and with a floor at one period of similar darkness, present the same phenomena? The fact that Archimedes and the many other similarly placed formations do not present this darkening, and that Plato alone of the many hundred lunar-walled plains does darken to this great extent, shows that there is something anomalous in the constitution of its interior, which is what selenographers contend. It is obvious, therefore, that the hypothesis framed by Mr. Proctor to account for the phenomena fails to do so when critically examined.

In his paper, Mr. Proctor refers, however, to what he considers direct evidence that the darkening of the floor of Plato is a mere effect of contrast, namely, that when examined with a small field of view, so as to shut out the greater portion of the surrounding bright environs, the interior of the walled plain showed no darkening towards full. This observation of Mr. Proctor's, if confirmed, would have been extremely important, because it would establish the fact of the darkening of the interior of Plato being merely the effect of contrast, and so make the general behaviour of the similarly situated walled plains the real anomaly. The observation of Mr. Proctor was confessedly imperfect, but the point required careful examination. For this purpose a numerous series of observations, with the field of view of the telescope purposely constructed to small dimensions, was undertaken, in some the field being reduced so small that only a portion even of the interior of Plato could be seen at once, so that the entire environs were shut out; in others, the border of the walled plain was included; and in others, more or less of the surrounding brighter regions in all less different sized fields were employed, a revolving diaphragm with apertures of different sizes being employed.

With these means a series of several thousand observations, extending over a period of nearly two years, was made, and not only of Plato, but of similarly placed or coloured formations. Nothing new, however, resulted; the interior of Plato went through the same course of darkening when observed through small fields of view as before, and the other regions exhibited their usual course of variations. The observation of Mr. Proctor was not therefore confirmed.

For the purpose of still more decisively settling this point, a piece of apparatus was constructed whereby—through the agency of shifting slides of blackened metal, with different sized apertures—any two or three different small regions of the moon could be seen isolated in the same field, all other portions of the surface being cut out. Moreover, by illuminating the upper side of the dark shutters the great contrast between the bright moon and a dark field could be subdued. In any case, however, as each of the two or three portions were seen at once, under exactly the same conditions, the effect of contrast was removed. By this means, from time to time, the floor of Plato was compared with other small portions of the lunar surface, so that any change would be at once detectable, and a numerous series of observations made. Under these conditions the darkening in the floor of Plato was as marked as ever, though all the effects of contrast with the surrounding bright highlands were entirely removed, for these bright highlands were shut out of view; and as both portions of the moon were thus placed under exactly similar conditions were they of the same brightness they should have so appeared when thus directly compared. By this means the reality of the darkening of the floor of Plato was incontestibly established, and shown to remain unaffected when the effects of contrast are thus eliminated.*

The explanation advanced by Mr. Proctor to account for the darkening of the floor of Plato is not therefore adequate for its purpose. Unquestionably it may have some effect, but whatever its amount may be it is too small to be sensible. Some small variations in the amount of the darkening of Plato, when observed by the various methods detailed, were also found to exist, but they were relatively very small in amount. These differences were, however, no more than were to be expected.

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* Since this was written a statement has appeared in a letter to an English periodical, from Mr. Proctor, to the effect that, whilst somewhere in America, some American gentleman—neither whose address nor name is mentioned—once told him that he thought no real darkening in the floor of Plato took place. This must be taken *quantum valeat*, and is only mentioned so that all published information on this subject may be referred to. A reviewer of my work on the Moon, in a very curt and *gauche* manner, contradicts the above statement, without giving any grounds for his behaviour. It is to be presumed he drew on himself for the numerous series of observations necessary to authorise a point blank denial of a result established by a long series of elaborate observations made with powerful instruments. Until these are published, however,—and nothing is known of them,—no more need be said of such a contradiction.

What, then, is the cause of this singular darkening in the floor of Plato? What reason can exist why that, as the solar altitude increases, the interior of Plato—unlike any other similar formation upon the surface of the moon—should alter in colour and brightness from a cold yellow-grey to a deep steel-grey, or almost black tint? It arises evidently, from what has been stated, from the anomalous conditions prevailing upon the surface of the interior of the wall plain. It is extremely improbable that these anomalous conditions should be an exceptional and fanciful configuration of the surface, and it is more natural to suppose that they exist in reference either to the condition or constitution of the surface. But however successfully the subject may have been pursued so far, here all further progress in elucidating this question is suddenly arrested,—no *matériel* exist for a further investigation. The observations now required are not those of simple observation with the usual astronomical appliances; what is wanted is special observations with special appliances, and these are at present not to be obtained. And, in fact, the great difficulties in the way of making these observations, and the powerful and costly instruments, and the experience required, effectually debar all except a few favoured astronomers from pursuing these observations. Thus, as in so many selenographical problems, patient observation establishes the existence of certain phenomena, but the elucidation of the meaning of the phenomena established is checked for want of special observations that are never made. For these selenographers have to appeal to those astronomers devoted to what has been termed astronomical physics, but they are too much engaged on, to them, more fascinating subjects to be able to assist selenographers.

Although, therefore, the means do not exist for saying, in one manner or another, this change results from this cause or from that cause, it is not fruitless to enquire what their studies have led selenographers to believe to be the cause of this change. The opinion, then, that appears to agree with the views of the most experienced selenographers who have studied the phenomena presented by Plato, is that the darkening of the floor of Plato results from an actual change due to heating action of the solar rays. Either from the volatilisation of some constituent of the substance composing the interior of Plato, or from some change in its constitution due to the heating effect of the solar rays, the floor darkens in colour. Now, there are more than one substance, known as a constituent of the crust of the earth, that thus darkens when submitted to heat,—some from loss of moisture, others from loss of other substances, and others again apparently from a molecular action. Selenographers cannot say which of these probably forms the floor of Plato, or rather to which of these does the substance probably forming the floor of Plato bear most resemblance; but there is every reason to believe that to one of them the floor of Plato bears a strong analogy.

It has been urged by Mr. Proctor (*loc. cit.*, p. 504) that under these conditions the maximum change on Plato would not occur until long after the sun had attained its meridianal altitude on Plato, whereas observation shows that this takes place shortly after this period. It can be shown that this is not necessarily the case, because the time when the effect of the solar rays reaches its maximum power depends very greatly on the action exerted by the lunar atmosphere. There is not merely a heating action only, but a loss of heat through various causes; so that the problem is complex, and involves the balance of action between the opposite effects. The question of the effect of the lunar atmosphere in modifying the action of the solar rays upon the moon, though one of very great interest, is one involving very great difficulties, more especially as observation and theory appear to be at variance in one or two points. The influence of the lunar atmosphere is, however, sufficiently great to remove the difficulty that has been urged on this point, just in the same manner that the action of the terrestrial atmosphere renders the hottest portion of the terrestrial day close after noon, rather than towards evening. The real power of this active agent, the lunar atmosphere, is rarely properly appreciated, and, in fact, the whole question of the atmospheres of the planets, as if it were forgotten that the whole subject is within certain limits as reducible to the control of the present powers of mathematical analysis as the motions of the planets themselves. In a future number this subject may be returned to, and the real power exerted by a lunar atmosphere on the phenomena presented by the moon, and on the condition of its surface, will be considered.

The instances that have been dealt with in the preceding pages will show that selenographers are not without strong evidence in favour of the opinion that has long been unanimously held by them, that processes of change are still actively at work upon the moon. It must not, however, be supposed that the above are the sole instances which they have recognised, because this is by no means true, only the difficulties in establishing others have led to their omission here. Dealing with a subject such as Selenography, it is only those who are familiar with all its details who properly appreciate the evidence in favour of or against its problems, —just as in the branches of mathematical astronomy, dealing with our satellite, it is only a proficient in mathematical astronomy who could appreciate the difficulties of the lunar theory, and feel a true confidence in the correctness of the manner in which they have been overcome. The difficulties in the way of making the true bearings of selenographical questions properly understood are greater than might be imagined; for even the very elementary fact that volcanic changes such as are now active on the earth would not be recognisable on the moon, in the present state of our acquaintance with the configuration of its surface, is not generally understood. When, however, the attention of astronomers is more generally directed to the study of the lunar surface, Science will be greatly the gainer, as it is there that the past and future of our earth is to be learnt.

II. EVOLUTION BY EXPANSION, VERSUS EVOLUTION BY NATURAL SELECTION.

By J. HUDDART.

THERE is nothing more difficult than to disembarrass the mind of a preconceived idea. A belief instilled in childhood and entertained through life, without a shadow of suspicion as to its truth, takes such firm hold of the imagination as positively to unfit the reason to judge impartially of any opinion opposed to that belief, however false and monstrous it may be.

The tradition of the fixity of species has become so deeply rooted in the human mind that only time, overwhelming evidence, and the untiring efforts of such pioneers of thought as Darwin, will ever eradicate the belief in it. He has already combated this belief with such success that he has considerably shaken the opinions on the subject of many eminent naturalists, while he has made more converts to the doctrine of Descent than is generally supposed. He was by no means the first naturalist to discard the old theory of Creation. He, moreover, disclaims to have been the first to have accounted for the gradual development of species by natural selection, stating that in this he was long preceded by Dr. Wells and Mr. Mathews. Owing, however, to the labour and thought he has expended in elaborating the theory, it will doubtless be always chiefly associated with his name. He considers natural selection to have been the main instrument in the formation of species, but allows that sexual selection, climate, geographical distribution, and other minor influences have had their share in the work.

The striking similarity in appearance, habits, and structure presented by many species of plants and animals first caused naturalists to doubt the "Fixity of Species," and to seek a reason for "Unity of Type" as it is called. According to Darwin, "Nothing can be more hopeless than to attempt to explain this similarity of pattern in members of the same class, by utility or the doctrine of final causes. The hopelessness of the attempt has been expressly admitted by Owen in his most interesting work on the Nature of Limbs. On the ordinary view of the independent creation of each being, one can only say that so it is;—that it has pleased the Creator to construct all animals and plants on a uniform plan: but this is not a scientific explanation."

The most obvious way of accounting for the phenomenon is by concluding that species possessing similar characteristics are related to one another. This is the doctrine of descent. We may go further, and presume that families, genera, and classes presenting points of similitude are also related.

But assuming the truth of these conclusions, it is evident that species vary and develop into other forms; also that their development is progressive, as it is well known that each geological era is characterised by higher types of fauna and flora than the preceding one.

There must be a cause for this gradual development of new forms. The agent that effects these changes is, according to the Darwinian theory, natural selection.

The theory of the origin of species by gradual development, or, more properly, by descent, is often regarded as identical with, dependent on, or so closely connected with that of natural selection, as to stand or fall with it. Darwin himself, in many instances, appears to consider development by descent or evolution as almost synonymous with development by natural selection. The theory of natural selection is, however, merely a method of accounting for or explaining the gradual development of species; and it may be sound or not, without affecting the validity of the theory of descent.

It is my object in this paper to compare this theory of natural selection with that which supposes the existence of an innate force,* and to show that the latter affords the more satisfactory explanation of the principal facts of natural history, physiology, palæontology, &c.

For this purpose I will assume the truth of the theory of the origin of species by gradual development. It has been so ably defended by Darwin, in his "Origin of Species," as to leave no new argument to be urged in its favour. The greatest difficulty which it presents, and perhaps the only one of any importance, is the absence of intermediate links or gradations between existing species: this he accounts for on the theory of the survival of the fittest only in the struggle for life, and by the imperfection of the geological records.

The inadequacy of the theory of natural selection to explain all the phenomena of natural history in a thoroughly satisfactory manner has undoubtedly caused many eminent naturalists to retain their belief in the old theory of creation.

Notwithstanding all the ingenuity of Darwin's arguments, and the vast amount of learning and scientific research he has brought to bear on the subject in support of his theory, a thoughtful reader—though much interested, and perhaps somewhat perplexed—must close his work on the "Origin of Species" with a feeling of dissatisfaction; he cannot realise that such haphazard means as natural selection and the survival of the fittest can alone have wrought such marvels as are exhibited throughout creation; nor, on the *primâ facie* view of the case, that they alone can have built up an ever-progressing fauna and flora, infinite in variety, but always subject to definite laws, always retaining their typical characters, while evincing adaptation the most perfect to the conditions of existence.

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* The term "Force," it must be clearly understood, is used in this paper, not in the sense of "Germ Force" or "Will Force," but to express merely a faculty of development; and that the word "growth" is used, not in the restricted sense of mere "enlargement," but in the more extended one of "progress" or "improvement."

It is not my intention to review the numberless objections to the theory of natural selection that have been urged by different naturalists, prominent among whom are Mivart, Nägeli, &c.; but to show how completely and fully the theory of an innate power of growth or expansion is borne out by the most important phenomena of morphology, and embryology taken in connection with palæontology.

In advocating the theory of expansion, I am prepared to admit that natural selection has probably been a subsidiary agent, and one of some importance; but the agency I believe to have mainly effected what Darwin ascribes to natural selection would seem to be a subtile force inherent in all organisms, too potent to be disturbed in its action to any considerable extent by extraneous influences, and destined to work certain definite results in a definite period of time; in short, a power of growth or expansion implanted in all organisms, when launched into existence, and extending through all time. This power of growth would also appear to develop in all organisms similar features, subject to the modifying effects of climate, conditions of life, &c., at the same stage of expansion.

I will consider this definition of the Innate Force in reference, first, to the phenomena of Morphology.

All plants and animals are classed according to their morphological characteristics, and not with reference to their habits, &c. The habits of different species belonging to the same class are often as different as it is possible to conceive, and they use homologous parts,—that is, corresponding portions of their structures made on the same model or type for widely different purposes. That is to say, species preserve their type no matter what be their conditions of existence. Conformity to type is thus the paramount law in the formation of species; adaptation to the conditions of existence a secondary law. Darwin considers the latter law to be the higher: were this the case we should not find the former asserting itself with unvarying persistence in the most diverse adaptations of homologous parts. Type is persistent through every adaptation. Adaptation is nothing more than a modification of type to suit certain conditions. Adaptation may be the result of natural selection, or it may not; but type cannot be, as its existence is incontestibly independent of, and has no connection with, the conditions of existence, which, however, wholly influence natural selection. In illustration of this I cannot do better than quote Darwin. When writing on the subject of morphology, he says—"This is one of the most interesting departments of natural history, and may be said to be its very soul. What can be more curious than that the hand of a man, formed for grasping, that of a mole for digging, the leg of a horse, the paddle of a porpoise, and the wing of a bat, should all be constructed on the same pattern, and should include similar bones, in the same relative positions? How curious it is, to give a subordinate though striking instance, that the hind feet of the kangaroo, which are so well fitted for bounding over the open plains,—those of the climbing, leaf-eating koala equally well fitted for grasping the branches of trees,—those of the ground-dwelling, insect- or root-eating bandicoots,—and those of some other Australian marsupials,—should all be constructed on the same extraordinary type, namely, with the bones of the second and third digits extremely slender and enveloped within the same skin, so that they appear like a single toe furnished with two claws. Notwithstanding this similarity of pattern, it is obvious that the hind feet of these several animals are used for as widely different purposes as it is possible to conceive. The case is rendered all the more striking by the American opossums, which follow nearly the same habits of life as some of their Australian relatives, having feet constructed on the ordinary plan. Prof. Flower, from whom these statements are taken, remarks in conclusion—'We may call this conformity to type, without getting much nearer to an explanation of the phenomenon;' and then he adds—'but is it not powerfully suggestive of true relationship, of inheritance from a common ancestor?'"

It is certainly suggestive of this, and of much more. It seems to show very clearly that morphological characters, referable to type only, do not vary with the conditions of existence.

A very remarkable instance of adaptation to the conditions of existence is exhibited by many species of flat-fish, which swim with the median plane horizontal, instead of vertical as in other fishes, and which, while maintaining their type, have heads actually distorted in a very singular manner to suit their habits.

Were natural selection the sole architect of animal life, it would be expected that members fulfilling similar offices would be similarly constructed, instead of conforming to the inexorable law of unity of type. But we find that any part of an organism, belonging to no matter what class, is capable of adaptation to any use, within certain limits, to suit the conditions of existence of its possessor; moreover, that homologous parts have often totally dissimilar functions in different species; for instance, the skull is supposed to be composed of metamorphosed vertebræ, and crustaceans having complex mouths have few legs, while those with simpler mouths have almost invariably more numerous legs; thus, even organs of locomotion may be converted into jaws. There is no reason, deducible from the theory of natural selection, why, in acquiring a more complex mouth, a crustacean should be deprived of some of its legs. It is simply a consequence of the laws of correlation of growth, and would seem to indicate that at a certain period of development the different systems of an animal are capable of modification, but not of extension.

This phenomenon of correlative growths is merely another form of conformity to type.

Darwin himself seems to perceive, in this correlation of growth, the presence of a higher power than mere natural selection. On the subject of domestication he says—"Hairless dogs have imperfect teeth; long-haired and coarse-haired animals are apt, as is asserted, to have long or many horns; pigeons with feathered feet have skin between their outer toes, pigeons with short beaks have small feet, and those with long beaks large feet. Hence if a man goes on selecting, and thus augmenting any peculiarity, he will certainly modify unintentionally other parts of the structure, owing to the *mysterious laws of correlation*."

What is this mystery but the existence of an innate force? What are these laws of correlation but the evidences of its existence?

The existence of rudimentary parts is essentially a conformation to type, and another phase of correlation of growth. It perhaps shows, more clearly than any facts yet mentioned, the nature of the principle of expansion. Rudimentary parts should be regarded not as organs or members in a state of transition, but as developments correlative to those of certain other portions of the organism that are in use; for it can abundantly be shown that rudiments of all kinds exist in almost every organic structure that not only are useless to the individual, but that never can have been, nor ever can be, of any use to the race. The only way of explaining the fact of their existence is by the supposition that organisms do not change their characteristics one by one, but all simultaneously by a slow process of growth, so that no one part can be behind another in its stage of development. Thus the dog-fish, skate, and other fishes of the kind have partially lost most of their fish-characteristics together. The bristles of the dog-fish and other cartilaginous fishes may be regarded as rudimentary fur, such as the seal possesses. Most apes have simultaneously lost the callosities, the cheek-pouches, the tail, the pointed ears, &c., peculiar to the monkey proper. The swinging of a man's arms in unison with the movements of his legs in walking has reference to correlative development.

Darwin regards rudimentary parts that are apparently disappearing as in a state of decadence through disuse; he fails, however, satisfactorily to explain the existence of those in a nascent state, but of no present use; still less of those in an apparently permanent condition—such as the mammæ of males. It is difficult to imagine that the sexes of Mammalia were ever so confused as to give rise to this phenomenon, especially when the sexes of their immediate progenitors are so perfectly defined.

There is another difficulty, with regard to organs in this state, that Darwin freely confesses has fairly baffled him; natural selection alone cannot account for it. He admits that some additional explanation, which he cannot give, is necessary to explain the still further reduction in size or complete obliteration of an organ that appears to have reached the limit of decadence from disuse, and that it is scarcely possible that disuse can go on producing any further effect after the organ has once been rendered functionless. The explanation of this case, as well as of all other cases of rudimentary growths, is obvious enough on the theory of correlative expansion. Thus it would appear that all parts of an organism and its characteristic features are functions of one another and of the whole organism, and all vary together. The symmetry of form, so noticeable throughout animate nature, is another result of the laws of correlation. Were natural selection permitted to mould the forms of life around us, uncontrolled and undirected by a supreme force, that of correlative expansion, shapes the most grotesque and monstrous would inevitably inhabit the globe.

Instances showing that type is independent of the conditions of existence, that rudimentary parts can be satisfactorily accounted for only by regarding their existence as due to correlation of growth or expansion, and that correlative expansion is a phase of the phenomenon of conformity to type, might be multiplied to any extent.

From these considerations, while agreeing with Darwin that community of type indicates descent from a common ancestor, *I consider that the type was not acquired by that ancestor, but was inherent in it, and still is inherent in its progeny.*

Type itself undoubtedly also develops and changes, but without reference to the conditions of existence or extraneous influences, and solely by expansion through age after age, expansion wrought by the mysterious inherent force performing its work in a set time with the exactness and regularity observable in all operations of Nature.

Modification of type to suit the conditions of existence must be more variable than type itself, as the same typical characteristics are common to a vast number of species adapted to most diverse conditions of life. There are comparatively few leading types, but an enormous variety of adaptations of each type. The type of an organism therefore changes very slowly in comparison to the alterations in its structure due to extraneous causes.

These conclusions, arrived at from the evidence of morphological phenomena, are borne out in a very striking manner by those of embryology, taken in connection with the facts of palæontology.

A study of these subjects also reveals to us more fully the nature of an organism.

The embryos or larvæ of all animals pass through many phases in the course of development. At certain stages the embryos of reptiles, birds, and mammals so closely resemble one another as to be undistinguishable. The embryo of the lower organisation is, in this class of animals, arrested in its growth, or more properly development, at an earlier stage than that of the higher; that is to say, the unformed embryonic mass does not at once acquire the characteristics of its species, but first assumes the form of a remote progenitor of its class, and expands through progressive stages till it arrives at the degree of development of its parents. In these stages the embryo frequently resembles extinct members of the class to which it belongs. In short, the embryo is developed by the same stages by which the race to which it belongs was developed in the course of successive geological eras; *i.e.*, the development-history of the individual is a recapitulation of the development-history of the stock to which the individual belongs. The various phases are so merged into one another, in the development or expansion during gestation, as somewhat to obliterate their characteristic traits and to render them indefinite, but still they indubitably accurately correspond to the stages of descent. Thus an organism is expanded in the short period of gestation or incubation in precisely the same manner as its progenitors have been expanded through thousands of generations.

What is the conclusion to be formed from these marvellous facts? That the organism is descended from ancestors possessing the characters presented by the embryo? Certainly this, and much more,—namely, *that there indubitably exists in all organisms an innate power of expansion by which at least the individual is developed*, for it is impossible that the egg (of a bird, for instance, during the period of incubation) can be acted on by extraneous modifying influences; and it is a fair and logical inference that the agent that produced the metamorphoses in the development of the race is identical with the agent which produces precisely similar metamorphoses in the development of the individual. What can be more easy to conceive than that each individual may be expanded in an extremely slight degree beyond the stage of development of its parents?

What has been remarked with respect to the embryonic development of the higher forms of life is equally applicable to the analogous phenomena of the metamorphoses of insects. The larval development is an expansion to a state of higher organisation, and takes place generally in the pupa, where no influence but an innate power can effect the changes.

We may not be able to trace descent in the embryo beyond a certain point, and some stages are in many species suppressed that are exhibited by others. But, reasoning by analogy and by the evidence of geological records, we may conclude that the progenitors of all organisms must be among the earliest, and therefore lowest, forms of life. Thus it would appear that the simplest known forms of life are endowed, equally with the unformed embryo or seed, with a power of growth or expansion, a faculty of being drawn out through certain stages which occur always in the same order and with the same correlative features. The germs of each and every system that make up the entire structure, of a bird for instance, exist as certainly in the lancelet (*Amphioxus*, lowest of the vertebrata) as in the egg of the bird at the commencement of the period of incubation; they are, so to speak, latent in each case. In the first case, the development of those germs will occupy millions of ages; in the second case, such age-development, having already been consummated, is recapitulated in the course of a few days.

I have stated that larvæ in their development ascend in the scale of organisation. Darwin says that some descend. If the fact of the descent be real, and not only apparent, it presents a very formidable difficulty. But, as Darwin also remarks, it is very difficult to determine what constitutes the higher organism. It certainly is not superiority in any particular quality, or greater perfection of any one organ. He says that the embryo in course of development generally rises in organisation, and that he uses that expression though he is aware that it is hardly possible to define clearly what is meant by the organisation being higher or lower. Considering that development is almost, if not quite, universally progressive, and not retrogressive,—in short, that progress is the established law of Nature,—the higher organism may, I think, safely be defined to be that which has been produced in the later stage of evolution.

Characteristics acquired by natural selection alone would supersede, permanently and entirely, earlier adaptations, as there is no reason deducible from the supposition that the formation of species is wholly due to the conditions of existence, to account for organisms reverting to ancestral types in their embryos or larvæ. Peculiarities of structure, the result of extraneous influences, when once lost or replaced by others, could not recur either in the embryonic state or at maturity without the exciting cause of the same extraneous influences. Ancestral type alone is reproduced in the embryo, and not the modification of type due to natural selection,—the grand leading characters, but not the innumerable adaptations.

Thus the phenomena of embryology, while strongly supporting the theory of descent with modification, decidedly militate against the supposition that all structural changes are due to natural selection.

Darwin, in quoting Mr. Lewes, remarks that the tadpole of the common salamander or water-newt "has gills, and passes its existence in the water; but the *Salamandra atra*, which lives high up among the mountains, brings forth its young full-formed. This animal never lives in the water. Yet if we open a gravid female, we find tadpoles inside her with exquisitely feathered gills; and when placed in water they swim about like the tadpoles of the common water-newt. Obviously this aquatic organisation has no reference to the future life of the animal, nor has it any adaptation to its embryonic condition; it has solely reference to ancestral adaptations,—*it repeats a phase in the development of its progenitors.*"

This is a case very much to the point. We cannot conceive a recurrence to ancestral adaptation due to natural selection without a distinctly beneficial result accruing from it, or without some direct cause for it. The recurrence to ancestral forms can only be a recapitulation of earlier types.

Having thus briefly reviewed the evidence of the leading facts of natural history and allied subjects, in reference to my theory, I will now arrange what deductions I have made from them, in such a form as to afford a clear appreciation of my position.

First, I have shown that type is persistent through every adaptation to the conditions of existence; or, in other words, that adaptation to the conditions of existence is independent of type. From this I infer that *conformity to type is due to a cause separate and distinct from that which effects adaptation to the conditions of existence;* the latter may therefore be the result of natural selection, but the former must be due to an innate power,—that is, natural selection may modify, but cannot originate type.

Secondly, I have pointed out that a power of expansion indubitably exists in all organisms,—that it produces in the individual a repetition of the metamorphoses that have occurred in the race. From this similarity of effect I infer a similarity of cause; *i.e., that the race was developed by a process of expansion, or unfolding.*

I have, moreover, shown that correlation of growth can be satisfactorily explained only on the supposition of correlative expansion. I have also indicated the impossibility of explaining the recurrence of organisms to ancestral types in their embryonic or larval stages, on the theory of natural selection.

I may here mention how strikingly the theory of an innate force harmonises with the well-known fact that similar forms of life have appeared simultaneously all over the world. This is accounted for by Darwin on the supposition that dominant species have a tendency to spread widely; but this does not account for the distribution of all species with sufficient rapidity to represent this phenomenon; for instance, terrestrial Mammalia would not migrate to any great extent, and very slight barriers would confine them to a limited area for generations.

The phenomenon is, however, decidedly suggestive of the gradual evolution of species by some agency acting at a uniform rate throughout the world.

The appearance at the present time, in similar climates, of plants and animals closely resembling one another, and yet distinctly different in continents or tracts separated by natural barriers, would indicate not so much relationship as expansion from a similar source by the same laws, the slight divergence of character being due to a difference in the conditions of existence.

I have not yet touched on the subjects of instinct, organs of high perfection, and the evidences of design: they do not appear to me to admit of the exact reasoning applicable to, and the strictly logical conclusions deducible from, the leading facts—the grand truths of natural history on which I have in this paper based my arguments in support of the theory of evolution by expansion.

Darwin is well aware of the difficulties they present to the theory of natural selection, and he has met these difficulties by arguments more admirable for their ingenuity than for their conclusiveness. Thus, he says of instinct,—"Many instincts are so wonderful that their development will probably appear to the reader a difficulty sufficient to overthrow my whole theory. I may here premise that I have nothing to do with the origin of the mental powers, any more than I have with that of life itself. We are concerned only with the diversities of instinct and of the other mental faculties in animals of the same class."

He here avoids raising a query that can, it seems to me, admit of only one answer.

The same difficulty occurs to him in connection with organs of extreme perfection, and is disposed of in the same way. Thus he says—"To suppose that the eye, with all its admirable contrivances for adjusting the focus to different distances, for admitting different amounts of light, and for the correction of spherical and chromatic aberration, could have been formed by natural selection, seems, I freely confess, absurd in the highest degree. When it was first said that the sun stood still and the world turned round, the common sense of mankind declared the doctrine false, but the old saying of *Vox populi vox Dei*, as every philosopher knows, cannot be trusted in Science. Reason tells me that if numerous gradations from a simple and imperfect eye to one complex and perfect can be shown to exist, each grade being useful to its possessor, as is certainly the case; if, further, the eye ever varies and the variations are inherited, as is certainly likewise the case; and if such variations should be useful, under changing conditions of life, then the difficulty of believing that a perfect and complex eye could be formed by natural selection, though insuperable by our imagination, should not be considered subversive of the theory. How a nerve comes to be sensitive to light hardly concerns us more than how life itself originated."

Not common sense only, but Reason rebels, when required to acquiesce in a theory which supposes that an eye, perfect and elaborately constructed in accordance with occult laws of physics, could have been formed by a series of accidental variations—as it would if required to believe that a philosophical instrument made by man could have been evolved by such means.

But there is no difficulty in allowing that the most perfect eye has been gradually developed from the most imperfect, provided we recognise the existence of an agent adequate to the accomplishment or achievement of such development.

The existence of the evidences of design in creation is so much a matter of opinion that it would serve no purpose to discuss it here ; if, however, it be once admitted that design is apparent in the different forms of life, the theory that natural selection has been the chief agent in their formation becomes untenable.

It is needless to point out how the above difficulties to the theory of natural selection tend to corroborate the theory advocated in this paper.

When Darwin evades more searching investigation of his difficulties by coming to the conclusion that to attempt to explain them more fully than he does would involve an inquiry into the origin of life itself: when he uses such expressions as "the mysterious laws of correlation ;" when, indeed, he assumes that an organism has the faculty of adapting itself to its condition of life, has the power of improving its members or organs, and altering them to serve for new purposes ; when he absolutely founds his theory on the supposition that an organism is endowed with the power, by a long series of efforts, and the inheritance of slight variations, of developing into a new and more highly organised structure, does he not tacitly admit the existence of an inherent force or energy, or, in other words, of a power of expansion ? External conditions would have no more effect on organic than on inorganic matter, were not the former endowed with a power of growth, development, expansion. Were organisms merely plastic there could be no progress, no improvement, no ascent to higher forms of life ; there might be variety or change, but there would be nothing new or essentially different. "Ex nihilo, nihil fit ;" in default of a power of expansion extraneous influences would have nothing to work upon, no energy to direct, no growth to mould to the changing conditions of life.

I have seen it stated that the theory of natural selection does not suppose the individual to be capable of self-improvement, but that purely fortuitous variations are rendered permanent by inheritance provided they happen to be improvements, and that thus only have the different forms of life been produced. But Darwin certainly believes that some variations are due to effort, or use and disuse, caused, however, entirely by the accidental circumstances of the conditions of life, and that variations that are inherited become characteristics of new species.* Although he has not expressly excluded the possibility of the action of some other agency, the general tenour of his writings is sufficient to convince us that in his opinion natural selection is sufficient to account for the origin of species. The only instances in which he appears to entertain misgivings on this point have been already mentioned.

The chief difficulty of the theory of expansion is the fact of the existence at the present time of the lowest forms of life. Several explanations suggest themselves:—

First. The germs of life may exist inactive for many ages, until activity is set up by some particular cause.
Secondly. The germs of life may be capable of spontaneous generation.
Thirdly. Certain germs may have a limited power of development.
Fourthly. Certain germs may be retarded or completely arrested in their development from some cause or other.

These suggestions are purely speculative; they are given merely to show that the difficulty admits of explanation in various ways.

I have chosen the word "expansion" as the one open to the fewest objections when used in its mathematical sense. The word "growth" would confuse the age-development with the growth towards maturity of the individual, from which it is quite distinct. The words "development" and "evolution" express only improvement or extension, but "expansion" conveys the idea of development by the action of some force or agent, or else extension according to a definite law, such as the expansion of a binomial into an infinite series.†

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* See Preface to second edition of "Descent of Man."

† The word "Expansion" has been chosen, also, to distinguish the theory from that of "Progressive Development" advanced by Lamarck, and which he admits to be merely a statement of observed facts, and from those that suppose the existence of an innate tendency to improvement and perfection; in short, to distinguish a process of "unfolding" from one of "addition."

In conclusion, I must remark that it appears to me to be more philosophical, and more in accordance with what we know of the operations of Nature, to believe that species were formed by rigid and unchanging laws, acting through all time, than to suppose them to have been evolved by a series of accidents.

It is, indeed, a tremendous thought, that the highest forms of life have been expanding to their present state of perfection through hundreds of ages; that all forms of life are now unfolding and expanding to higher types,—expanding not by uncertain and fortuitous means, but by laws as immutable and inexorable as those which govern inanimate matter; that the great scheme of evolution had been fully planned and matured when the first minute germs of life, with their mighty destinies, came into active existence in a world which was then a wilderness, but which has since become full of glorious life, of forms most wonderful and beautiful, evolved by the mysterious power acting with ceaseless energy through all the bygone ages.

The imagination is dazzled in the contemplation of results so stupendous from a beginning apparently so insignificant; but the reason must needs bow before the great weight of evidence, and must admit the inevitable conclusion that in every living thing there is a force that for ever works upwards and onwards, that retrogression and decadence are impossible, save for a transient wave of apparent degeneration that seems occasionally to sweep over a race, and which is generally attributable to a temporary extraneous cause.

It would seem that Tennyson, with the divine inspiration of the poet, had some dim conception of the great truth when he wrote—

"A monstrous eft was of old the Lord and Master of Earth,
For him did his high sun flame, and his river billowing ran,
And he felt himself in his force to be Nature's crowning race.
As nine months go to the shaping an infant ripe for his birth
So many a million of ages have gone to the making of man:
He now is first, but is he the last? is he not too base?"

III. THE PORT OF YMUIDEN.

By F. C. Danvers, Assoc. Inst. C.E.

FEW people will recognise in the above name the great and important engineering work which has, since 1865, been under construction in Holland, and which has been more familiarly known as the "Amsterdam" or "North Sea Canal." Since the construction of the Suez Canal no hydraulic work of greater, or even equal, importance has been undertaken, and its successful completion reflects no little credit upon Sir John Hawkshaw and the able engineers who have been associated with him in the undertaking. A brief retrospect will show the necessity that existed for this work.

In the sixteenth century Amsterdam stood first of all the commercial cities of Europe. Its prosperity, however, gradually began to decline, partly from the rise of other ports, but principally from the difficulties of navigation caused by the silting up of the Zuyder Zee, and above all by the formation of the Pampas Bar. Large vessels could, in consequence, no longer get up to Amsterdam with their cargoes, but were obliged to discharge outside the bar, when they were floated over it by means of "camels," which, when the water was pumped out of them, raised the vessel with them. The trade of Amsterdam was fast being diverted to Rotterdam, but in 1819 steps were taken to avoid the difficulties of the Zuyder Zee by the construction of the North Holland Canal, which extends from Buiksluyt, opposite Amsterdam, to the Helder, a distance of 51 miles; and this canal has since been extensively used by vessels of large burden seeking the North Sea. It is, however, difficult of passage in winter, and consequently it was at length determined to connect Amsterdam with the North Sea direct by means of a canal passing through the Wijker-meer and the Ij, by the shortest possible route, by the adoption of which the distance to the sea is shortened by no less than 36 miles.

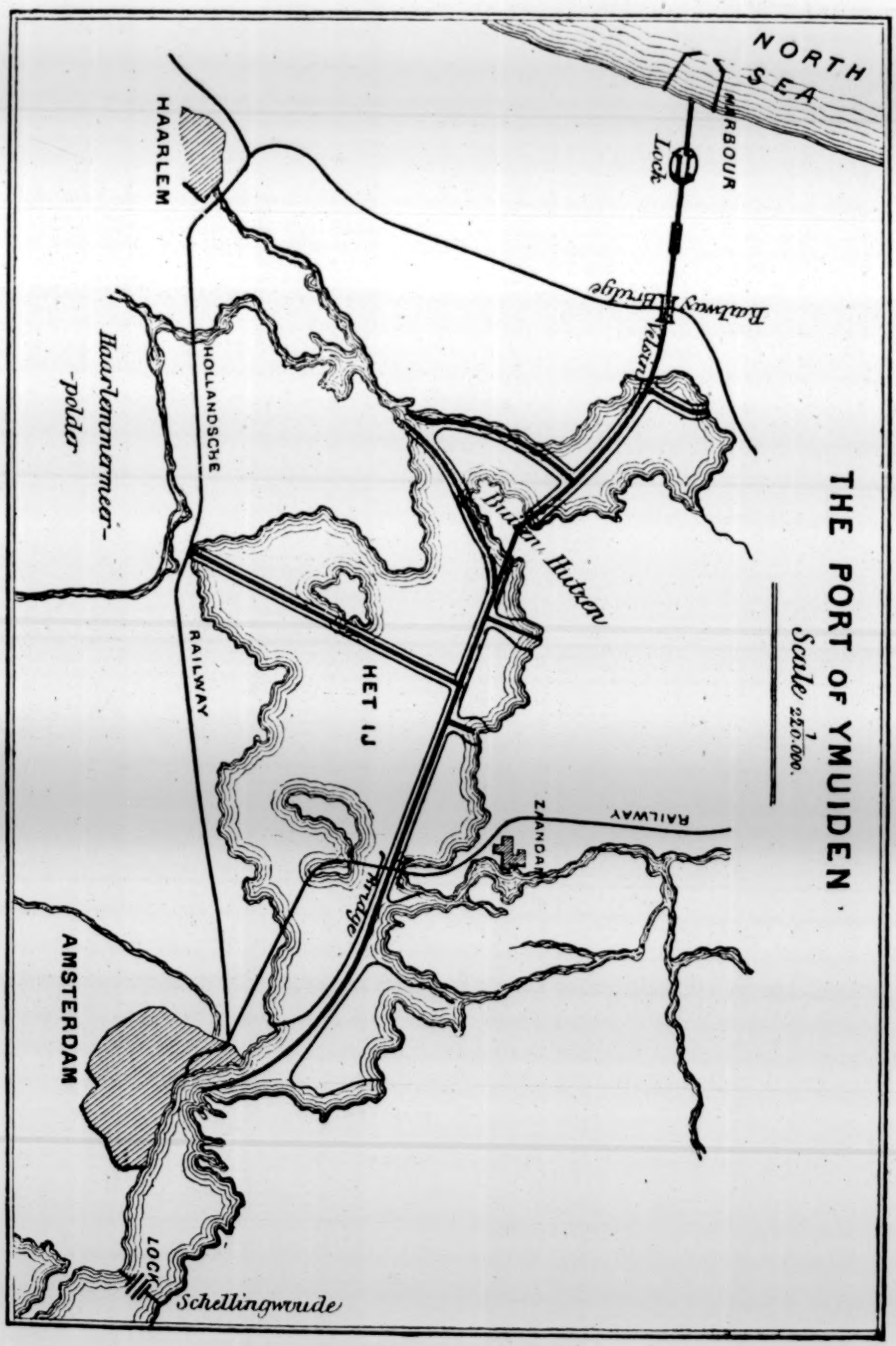
This work was commenced in March, 1865. It consists of two parts, viz., a harbour in the North Sea, at the mouth of the canal, and the canal itself.

The harbour was designed by Sir John Hawkshaw. It consists of two piers stretching out into the sea to the depth of about 26 feet below the level of low water. These piers at their junction with the land, at the foot of the downs on the beach, are 1312 yards distant from one another. Thence their directions converge, so as to make an angle of 77 degrees with their base line, and at 1312 yards from the shore they are 721¾ yards apart. From this point they begin to converge more rapidly, so that, with an additional length of 377 yards,—making 1689 yards in total length,—they terminate 284 yards apart at the harbour mouth.

The piers consist of a solid sea wall, 27 feet wide at top, and increasing in thickness downwards by one-seventh of their height. They are formed as follows:—First of all a foundation of basalt rock was thrown into the sea, about 40 yards wide and 1 yard in thickness. On this a superstructure is raised, formed of concrete blocks, varying from 4 to 10 tons in weight. The rubble foundation was levelled by divers, by whom also the blocks below low water were fitted, without any cement in their joints, the blocks being let down to their site by a powerful steam crane called a "Titan." From low water the blocks are laid in cement; for the lowest joints—which are only a few hours above water—a quick-setting Medina cement was used, and for the higher joints Portland cement. The top of the piers is generally 13 feet 6 inches above the Amsterdam pile (datum). The parapet on the top of each pier consists entirely of concrete, moulded on the spot in frames erected *in situ*. Behind this is a pavement of brick in cement, supported by a band of granite along the inner top line. In the construction of each of the two piers there has been used 433,600 yards of concrete blocks, 77,700 tons of cement, 229,000 cubic yards of shingle, 130,900 cubic yards of basalt, and 19,035 cubic yards of broken bricks, the last-named being used in the formation of the concrete blocks placed in the middle of each pier.

In order to afford further protection to the piers against the force of the waves, a slope of rubble rock has been thrown into the sea on the outside of each pier, the lower blocks of these wave-breakers being 10 tons each in weight, and those above low water about 20 tons each. The harbour will afford berths for three hundred large vessels.

The canal, in passing from the harbour through the downs, runs eastward at first in the direction of an arc of a circle, with the concave side turned southwards, for a distance of 1300 yards, at the end of which are the North Sea locks. The curved form of the canal in this portion serves principally to protect the locks from the force of the waves. From this point the canal runs for some distance in a straight line, and afterwards sometimes in curves, with occasional straight reaches, to Amsterdam—a total distance of 25,919 yards.


THE PORT OF YMUIDEN.
Scale 1/220.000.
NORTH SEA
HARBOUR
Lock
Railway Bridge
Velsen
Buiten Huizen
HET IJ
RAILWAY
ZAANDAM
Bridge
HAARLEM
HOLLANDSCHE
RAILWAY
Haarlemmermeer-polder
AMSTERDAM
LOCK
Schellingwoude

Two sections of the canal pass through dry land; the one from the North Sea to Wijkermeer, at Valsen, 6540 yards long; and the other across the peninsula of Beutzenhuisen, 875 yards in length. The portion of the canal passing through the Wijkermeer has a length of 4779 yards, and that through the Western Ij one of 13,725 yards.

The canal is formed with a bottom width of 88 feet 6 ins., and a surface width of 207 feet, and when finally completed it will be over 23 feet in depth. These dimensions continue throughout 22,966 yards in length of the canal, measured from the North Sea, after which the width gradually increases until it joins the "diep" near Amsterdam: here the dykes turn away from the canal, the northern one running in a straight line to Buikslooterham, and the southerly one in a curved line to Amsterdam.

The earth excavated from the land sections of the canal is conveyed in barges, and deposited on the muddy bottom of the lake to form dykes on either side, where it passes through the Wijkermeer and the Ij. These deposits are then covered with clay, and as soon as the bank appears above the surface of the water, and is formed, the slopes are protected from the wash of the waves with fascines. The canal channel is then dredged between the banks so formed, for which purpose steam dredgers are employed with self-acting discharging apparatus. This apparatus consists of a vertical cylinder fitted on to the steam dredger, into which the excavated mud is thrown. On the lower end of this cylinder a horizontal centrifugal pump-wheel, of about 40 ins. in diameter, works, which is driven by the engine of the steam dredger, at a speed of 230 revolutions a minute, forcing the mud from the cylinder, in a semi-fluid state, into a floating tube, through which it is carried away. The floating tubes consist of wooden cylinders of about 50 feet in length and 15 inches in diameter, joined together by flexible leather couplings. These float on the surface of the water for distances of from 750 to 800 feet, the ends being carried through and sometimes over the previously-made sandy dykes, behind which a jet of muddy water is discharged, containing from 40 to 50 per cent of solid matter, which thus becomes deposited over a considerable surface. From 1200 to 1500 tons of sand is raised by one dredger, with a 20-horse power engine, per day.

In order to connect the different navigation and drainage locks and sluices along the borders of the Ij with the main canal, several branch canals have been constructed, of varying sections, and having an aggregate length about equal to that of the main canal. The total length of dykes constructed is about 40 miles, and the excavation from the canals has amounted to about 13 millions of cubic yards of sand and mud.

The spaces outside the canal, being pumped dry, become suitable for meadows, or, as they are called "polders." In order to keep them dry from percolation or drainage, pumping-engines are placed at certain distances along the canal, which drain them and empty the water into the canal. The total amount reclaimed, and to be reclaimed, is 12,450 acres. These polders continue along the canal side until the sand dunes are reached at 3 miles from the west coast.

The canal is crossed at two points by railway bridges, but the most important engineering works are the locks at either end of the canal, which deserve a brief description here.

As has been already stated, the North Sea locks are placed 1300 yards from the western end of the canal. In these, special precautions were necessary to protect them from high tides. The locks are unusually high and strong, and are provided with extra gates, to be used when the tide rises above a certain height. Here there are two locks and a sluice, the larger of the two locks being capable of admitting a vessel 393½ feet long and 59 feet wide, with a depth over sill of 28 feet 8 inches at ordinary low water. The walls of this lock are of brickwork, the exposed faces of which are of pressed bricks, and the rest of the work of ordinary hard burned bricks. In the case of the smaller lock an earthen slope supplies the place of the southern wall along the lock pond.

The Zuyder Zee locks at Schellingwoude have been named the "Orange Locks," after the heir apparent to the throne of Holland. Side by side in the long dyke, which is nearly a mile long, are three locks for ships, one sluicing lock, and three masonry channels for pumping out, by steam, the water when the tide in the Zuyder Zee is higher than the water in the canal. The largest ship lock is 315 feet long and 59 feet wide; the length of the other two is 239 feet, and their width 46 feet. These locks are built on a foundation of 8896 piles, and the dyke rests on mattrasses of fascines laid on the mud. The dyke is composed of sand and clay laid on fascines, and faced on the sea side with blocks of basalt and granite covered with clay. These locks have altogether twenty-seven pairs of gates, of which eleven pairs are of iron and sixteen of wood. The lock walls are built exclusively of ordinary brickwork, in the same manner as those of the North Sea locks.

It will thus be seen that, although the canal itself is shorter, the works on it are considerably more important than anything that occurs on the Suez Canal. The "Orange Locks" were opened by the King of Holland in 1872, and the completed canal was opened by His Majesty on the 1st of November last, when he ordered that it should be known by the name of "The Port of Ymuiden." The total cost of this stupendous work has been about two millions sterling.

IV. ANIMAL GEOGRAPHY.*

ONE of the most striking distinctions between the old and the new school of Natural History is the greatly increased amount of attention paid in the present day to the *locality* of every species. Our predecessors, if any specimen had not been derived from their own country, quietly dubbed it "exotic." Any creature from a tropical climate was labelled as a native of "the Indies,"—which might include either Venezuela, Hindustan, or New Guinea. To the modern naturalist, on the contrary, an accurate knowledge of the locality of every specimen he examines is a point of the first moment. Without this he regards it in much the same manner as a lawyer looks upon an unsigned document. "The structure, affinities, and habits of a species now form only a part of its natural history. We require also to know its exact range at the present day and in pre-historic times, and to have some knowledge of its geological age, the place of its first appearance upon the globe, and of the various extinct forms most nearly allied to it."

But though the correct locality of each species is now recorded in every systematic work on natural history, though local faunæ have been compiled, and attempts made at a general classification of the animal world from a geographical point of view, a work was still wanting which should com-

* The Geographical Distribution of Animals, with a Study of the Relations of Living and Extinct Faunas as elucidating the Past Changes of the Earth's Surface. By ALFRED RUSSEL WALLACE. London: Macmillan and Co. Opening Address of the Biological Section of the British Association, 1876. By the President, ALFRED RUSSEL WALLACE.

bine and harmonise the mass of unconnected facts ascertained, and which should not merely propose an arrangement, but should demonstrate it by a careful and exhaustive analysis. This deficiency has been supplied by Mr. Wallace in a manner which must greatly enhance the well-merited esteem in which he is held by naturalists. The result is a work which in its department has no equal in any language, and which must at once be received as the text-book of zoological geography.

It may, at the first glance, appear an easy matter to determine the geographical distribution of the animal kingdom. We have only, it is said, to take a census of species in every country, to compare the returns, and to arrange our divisions accordingly; but the moment we make the attempt difficulties spring up on all sides. We require a trustworthy classification of animals, so that we may know what forms can be legitimately included under each species, genus, or family. We must then decide whether our classification is to be positive or negative, founded on the presence or on the mere absence of certain groups. Our own view, like that of Mr. Wallace, is that mere negative characteristics can have but very limited value. The extirpation of certain striking forms of life in a given island, whether effected by human agency or by natural causes, cannot give such island a higher rank as a zoological province than it had before. To distinguish two regions, *a* and *b*, we must be able to show that each contains something which is wanting in the other. Why, for instance, are the claims of Australia to rank as a distinct primary region so universally allowed? Not from the mere absence of monodelphic mammals, whether Carnivora, Rodentia, Ungulata, and the like, but because, in the stead of all these, there are didelphic groups which to some extent replace, or at least simulate, the monodelphic orders and families. This brings us to another fundamental principle,—the higher the rank of the group present in one country and absent in another, the more fundamental is the distinction between them. Thus two adjacent islands might contain not a single Lepidopterous species in common; yet if all the species belonged to genera common to both islands we should rank both in the same region, sub-region, province, and district. But suppose that they had no genera or no families in common, we should consider it necessary to refer them at any rate to distinct sub-regions. If, again, the very orders are distinct, as is the case if we compare the mammals of Australia with those of the rest of the world,* we have before us a distinction of the highest order. But here is a fresh difficulty: it is only Australia which offers us so sharp a demarcation, and even this extends merely to the Mammalia: its birds and insects, though very distinct, not being separated from those of other parts of the world by so broad a boundary line. In separating region from region we cannot always avail ourselves of characteristics absolutely equal in value. This, as we shall afterwards see, has led some systematists to maintain that Australia and South America are marked off from each other and from the rest of the world by features more striking than those presented by any other region. We must therefore call in another principle, already shadowed forth in the admission that mere poverty of species cannot constitute a zoological region. We must take into consideration richness and variety of forms, as well as speciality. Nor must we insist upon being able to prove that all our primary divisions are of precisely equal rank. Nature will not adapt itself to our systematic classifications, whether geographical or morphological. Look, *e.g.*, at our use of the term "order." It is applied equally to two such groups as Carnivora and Marsupialia. It must be admitted that the latter comprises at least four groups which, if more developed, might claim to rank as distinct orders. Or let us look at that vast assemblage of animated beings known as the "order" Coleoptera, but containing carnivorous, omnivorous, frugivorous, and lignivorous groups, differing widely as well in structure as in habits. Were they bulkier creatures, would not the "stirps" Geodephaga be entitled to the position of an order equivalent and parallel to Carnivora? Thus we see that our morphological groups, as well as our geograpical regions, are by no means equal in value.

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* With the exception of the opossums of North and South America.

But to return: the question next arising concerns the foundation of our regional division. Shall it be founded upon the consideration of some one sub-kingdom or class, and, if so, upon which? Mr. Wallace, like some of his predecessors, takes the Mammalia as his standard, and only calls to his aid the distribution of other groups to determine doubtful points, or by way of corroboration. We cannot help thinking that insects have a higher claim to be selected for our guidance. They form, so to speak, the round numbers of the world's animal species, all other tribes and classes being in comparison a mere fractional amount: they are rarely purposely introduced by man into foreign countries, and the few which follow him parasitically, such as the cockroach and the house-bug, are well known. If imported in articles of commerce they prove, as a rule, incapable of maintaining themselves, and soon disappear. Like the Mammalia, their means of dispersal are mainly dependent upon "the distribution of land and water, on the presence or absence of lofty mountains, desert plains, and great forests." Strange as it may seem, we can also trace their existence and distribution in remote geological epochs, and can identify genera in the tertiary and families even in the palæozoic period. It is true that no part of the world would be so sharply demarcated by its entomological fauna as is Australia by its mammalian forms of life; but this might not be wholly a disadvantage. Yet whilst we wish that an attempt might be made to draw up a system of animal geography based upon the distribution of insects, we are strongly inclined to believe that the main results of such an undertaking would confirm the labours of Mr. Wallace. Even plants will doubtless be found to conform to the same arrangement. "The floræ of tropical America, of Australia, of South Africa, and of Indo-Malaya, stand out with as much individuality as the faunæ, while the plants of the Palæarctic and Nearctic regions exhibit resemblances and diversities of a character not unlike those found among the animals."

Before entering upon an examination of the system of Mr. Wallace we may find it useful to take a brief survey of the divisions proposed by earlier authorities. The first attempts in animal geography are due to Fabricius, the eminent entomologist. He divides the world into eight sections: the Indian, comprising the tropical regions of both hemispheres; the Egyptian, including the northern subtropical lands, apparently in the new as well as in the old continent; the Mediterranean Islands, with southern Europe, and a part of Asia Minor; the North European; the North Asiatic; the North American; China, with Japan; and, finally, all mountains throughout the globe which reach the level of perpetual snow. It does not appear that Fabricius ever made any attempt to demonstrate his theory, which must inevitably break down under even the most superficial analysis, and which merely serves to prove how little attention must have been paid in his day to the localities of animal species. Latreille divides the world into "climates," each extending 12° of latitude by 24° of longitude. He does not, however, express himself satisfied that each of these plots is characterised by a distinct fauna. Kirby maintains that the limits of animal species are fixed not by isothermal lines,* but by the will of the Creator. He thus withdraws the subject entirely from the domain of Science, forgetting that Absolute Reason will work not arbitrarily, but according to fixed laws, even if the human intellect should not be equal to the task of their discovery.

Dr. Prichard adopts, as his zoological provinces, the Arctic, the Temperate, and Equatorial regions of the old and new continents; the Indian Archipelago; New Guinea, with New Britain, New Ireland, and the island groups of the Pacific; Australia proper; and, lastly, the southern extremities of America and Africa. This classification might be very briefly dismissed if it did not, at first sight, seem to anticipate certain views put forward by Mr. Wallace in his earlier writings, and developed in the present work. He finds that the respective faunæ of the western and eastern portions of the great Malay Archipelago differ essentially: hence he places the former group in his "Oriental" and the latter in his "Australian" region, drawing his line of demarcation between Borneo and Celebes. Whether Dr. Prichard's boundary falls in the same place, or, rather, more to the eastward between the Moluccas and New Guinea, it is evident that he considers the distinction between the Indian islands and New Guinea of no higher rank than that between the former and the south-eastern portion of the Asiatic continent, or than that between the latter and Australia. On the other hand, Mr. Wallace clearly demonstrates that widely as Australia differs from New Guinea in climate, soil, humidity, and state of surface, their respective faunæ show a well-marked affinity. New Guinea and Borneo, almost identical in their meteorological conditions, are decidedly distinct in their forms of animal life. Hence we must decide that Mr. Wallace has not been anticipated by Dr. Prichard, and that the latter was evidently not aware of the importance of the truth which he had approached.

Swainson's arrangement has at least the merit of not requiring any novel terminology. His five grand divisions are simply Europe, Asia, Africa, America, and Australia. How a man of his reading and research could succeed in persuading himself that the zoological distinction between Europe and Asia exceeded, or even equalled, those between Central Asia and India, or North and South America, respectively, might be an interesting puzzle for the laboriously idle. Like Kirby, Swainson supposes that the various groups of organic beings were originally placed by the Creator in certain regions for which they are peculiarly and exceptionally adapted. How completely this hypothesis is at variance with facts requires no further demonstration.

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* It will be observed that the boundaries of Latreille's regions are not necessarily isothermals.

Mr. Wallace, as the basis of his arrangement, adopts the six regions originally proposed in 1857 by Dr. Sclater. This view, at first established merely on a study of the distribution of birds, has since been applied by its author to the mammals, and by Dr. Günther to reptiles. The regions are—the Palæarctic, embracing the eastern continent from the Icy Ocean down to the Sahara, the Indus, and the Himalaya; 2nd, the Ethiopian, including all Africa south of the Great Desert, the tropical portion of Arabia, the islands of Madagascar, Mauritius, Bourbon, the Seychelles, and others in the Indian Ocean, but excluding the Azores, Madeiras, Canaries, and the Cape Verde group; 3rd, the Oriental region, comprising India, both hither and farther, along with the south-eastern portion of China, Ceylon, the Andamans and Nicobars, the Sunda Islands up to the Straits of Macassar, Hainan, and Formosa, and probably the Philippines; 4thly comes Australia, with New Guinea, the Moluccas and Celebes to the westward, and New Zealand and Polynesia to the south and east. Next follows the Neotropical region, or South America, with the West Indies, Central America up to the southern slope of the great Mexican table-land, and the Galapagos. Lastly we have the Nearctic region, including the whole of North America from the Mexican table-land to the furthest limit of animal life in the Polar regions.

Our first thought concerning this arrangement is that its nomenclature is unhappy. Four, if not five, of the names do not at once tell their own tale. Instead of Neotropical and Nearctic, it would surely be simpler to say West-Southern and West-Northern, or even South-American and North-American. The term "Oriental" might be supposed applicable to Persia, Arabia, and Syria, and may therefore be usefully replaced by the name "Indian," as originally proposed by Dr. Sclater. In like manner we would substitute "East-Northern" for "Palæarctic," and "African" for "Ethiopian."

But these six regions, howsoever named, are not universally accepted by naturalists. Prof. Huxley points out that the Australian and Neotropical regions differ more widely from the other four above mentioned than do these latter respectively from each other. Hence if we take Australia, South America, and all the rest of the world, which he calls

"Arctogæa,"—Arctic land, a rather grotesque name for a stretch of country which would have to include the Cape of Good Hope,—we should have only three primary regions, nearly equivalent. The same author also suggests that the peculiarities of New Zealand may perhaps justify its claim to rank as a primary region. Mr. Murray, in his "Geographical Distribution of Mammals," assumes four primary regions,—the Palæarctic of Dr. Sclater, with the addition of the Sahara and Nubia; the Indo-African, embracing Dr. Sclater's Oriental and Ethiopian regions; the Australian; and the American, including South as well as North America.

Mr. W. T. Blanford proposes to call the Oriental region of Mr. Sclater the Malayan, as being most highly developed in the Malay countries. He doubts whether India proper belongs to this region at all, and considers that it has derived a great part of its fauna from Africa.

Mr. E. Blyth, basing his classification upon mammals and birds, seeks to establish seven primary divisions:—the Boreal, comprehending the Palæarctic and Nearctic regions of Dr. Sclater, in addition to the West Indies, Central America, and the Andes down to Chili and Patagonia. Next comes the Columbian region, embracing the residue of South America. His Ethiopian region, in addition to Africa, comprehends Arabia, the south of Syria, the plains and table-lands of India, and even the northern half of Ceylon. Next follows the Lemurian region, comprising Madagascar and the adjacent island groups. Mr. Blyth's "Austral-Asian" region agrees with Dr. Sclater's "Oriental" region if the greater part of India proper is cut off. Finally, Dr. Sclater's "Australian" region is divided into two portions of equal rank—Melanesia, including Australia proper, New Guinea, and Celebes; and Polynesia, comprising the South Sea Islands and New Zealand.

Mr. J. A. Allen, again, assumes a "law of circumpolar distribution of life in zones," and divides the world into eight "realms,"—the Arctic; the North Temperate; the American Tropical; the Indo-African Tropical; the South-American Tropical; the African Temperate; the Antarctic; and the Australian. The North Temperate is again subdivided into the American and the Europæo-Asiatic regions; the Indo-African into the African and Indian regions; and the Australian into the Tropical Australian and the South Australian, with New Zealand. His other realms are not subdivided.

It is evidently disheartening to see such an utter want of accordance among authors who have certainly given this important subject their careful attention. No one indeed has proposed to unite Australia with any of the other primary zoological regions, or to dissever from it the eastern portion of the Malay Archipelago—the Austro-Malayan region of Mr. Wallace.* But short of this it would almost seem as if zoo-geographers had been "ringing the changes" on the possible number of arrangements. Even with the boldly isolated Australian region strange liberties have been taken: thus while one classifier would confer upon New Zealand the rank of a coequal primary region, another regards it as fit for amalgamation with South Australia, in contradistinction to the northern or tropical half of that continent.

As to most of these modifications of Dr. Sclater's original views, we think that Mr. Wallace is fully justified in their rejection. The fusion of South with North America, or that of the latter with the West Indies, Chili, Patagonia, Central America, Europe, and Asia, seems to us to involve the neglect of important distinctions and of plain affinities, and to offer great practical inconvenience. Mr. Wallace well remarks—"There can be little use in the knowledge that a group of animals is foundin the Boreal region, if their habitat might still be either Patagonia, the West Indies, or Japan." Concerning the proposal of Prof. Huxley—latterly adopted by Dr. Sclater—to consider New Zealand as a primary province, our author reminds us that "it is absolutely without indigenous Mammalia, and very poor in all forms of life, and therefore by no means prominent or important enough to form a primary region of the earth."

"It may be as well here to notice what appears to be a serious objection to making New Zealand, or any similar isolated district, one of the great zoological regions, comparable to South America, Australia, or Ethiopia, which is that its claims to such distinction rest on grounds that are liable to fail. It is because New Zealand, in addition to its negative merits, possesses three families of birds (Apterygidæ living, Dinornithidæ and Palapterygidæ extinct), and a peculiar lizard-like reptile, *Hatteria*, which has to be classed in a distinct order, Rhyncocephalina, that the rank of a region is claimed for it. But supposing, what is not at all improbable, that other Rhyncocephalina should be discovered in the interior of Australia or in New Guinea, and that Apterygidæ or Palapterygidæ should be found to have inhabited Australia in post-pliocene times (as Dinorthidæ have already been proved to have done), the claims of New Zealand would entirely fail, and it would be universally acknowledged to be a part of the great Australian region. No such reversal can take place in the other regions, because they rest not upon one or two, but upon a large number of peculiarities of such a nature that there is no room upon the globe for discoveries that can seriously modify them. Even if one or two peculiar types like Apterygidæ or *Hatteria* should permanently remain characteristic of New Zealand alone, we can account for these by the extreme isolation of the country and the absence of enemies, which have enabled these defenceless birds and reptiles to continue their existence, just as the isolation and protection of the caverns of Carniola have enabled the *Proteus* to survive in Europe. But supposing that the *Proteus* was the sole representative of an order of Batrachia, and that two or three other equally curious and isolated forms occurred along with it, no one would propose that these caverns or the district containing them should form one of the primary divisions of the earth. Neither can much stress be laid on the negative peculiarities of New Zealand, since they are found to an almost equal extent in every oceanic island."

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* As an additional proof of the close connection between New Guinea and Australia we may mention that M. Bruyn, of Ternate, has obtained a new species of *Echidna* from the mountains of Arfak, in the former island. The only two allied species hitherto known are confined to Australia proper.

As regards Prof. Huxley's tripartite arrangement—Australia, South America, and Arctogæa—Mr. Wallace urges that the comparative importance or equivalence of value of two or more zoological provinces is very difficult to determine. "It may be considered from the point of view of speciality or isolation, or from that of richness and variety of animal forms. In isolation and speciality, determined by what they want as well as by what they possess, the Australian and Neotropical regions are undoubtedly each comparable with the rest of the earth. But in richness and variety of forms they are both very much inferior. and are much more nearly comparable with the separate regions which compose it."

It might possibly, however, be contended that Mr. Blyth is right in claiming for Madagascar and its adjacent islands the rank of a primary region, instead of viewing it, with Dr. Sclater and Mr. Wallace, as a sub-region of "Ethiopia." True its extent is very trifling compared with any of the other regions, but there are some grounds for regarding it as the mere fragment of a former continent. It possesses twelve families of terrestrial Mammalia (or only two fewer than the Australian region), three of which are peculiar, whilst the Lemuridæ—although extending to continental Africa, the Malay Islands, India, and China—have evidently their metropolis in Madagascar, where they take the place of the monkeys. Of its twenty-seven genera and sixty-five species, no fewer than twenty genera and all the species are peculiar. Out of its one hundred and eleven species of land birds only twelve are identical with species inhabiting the adjacent continents, and not one of the exclusively African families is represented in Madagascar. The gigantic extinct bird Æpyornis, three species of which have been discovered, forming the family Æpyornithidæ, as far as is known was peculiar to Madagascar and the adjoining islands. Among reptiles it contains none of the African *Colubers*, but it has three genera—*Herpetodryas*, *Philodryas*, and *Heterodon*—which are only found elsewhere in North and South America, whilst the Lycodontidæ and Viperidæ, both well developed in Africa, are here absent. Its insect affinities are largely Oriental, Australian, and South-American, the African element being represented rather by special forms belonging to West Africa or South Africa than by such as are common to the whole Ethiopian region. Amongst the Lepidoptera the beautiful diurnal moth *Urania* occurs here, the remaining species being found only in the Neotropical region. Of the twenty-three Cetonian genera known in Madagascar, two alone—according to the "British Museum Catalogue"—are represented elsewhere. None of the characteristic *Cicindelas* of Africa are here met with, and with the *Carabs* the case is almost similar. Having regard to these facts, most if not all of which are duly recorded by Mr. Wallace, it certainly seems that "Lemuria," if not a primary region, differs more widely from the other Ethiopian sub-regions than they do from each other, or perhaps even than do the respective sub-regions of any other region.

The subdivision of the six primary regions is the next question. Mr. Wallace proposes in every case four sub-regions, a numerical agreement which, if fully borne out by facts, is exceedingly curious. Doubts, however, may be entertained both as regards the number and the boundaries of these subdivisions. In the Nearctic or North-American region the four districts are—the Californian, consisting of Upper California and the narrow strip of country to the northwards between the Rocky Mountains and the Pacific; the Rocky Mountains, embracing Lower California, the table-lands of Mexico, and the mountainous territories extending northwards towards the British frontier; the Appalachian, including the valley of the Mississipi and all the eastern and southern States of the American Union; and, lastly, the Canadian, comprehending British North America (except perhaps Columbia), the former Russian territory, Greenland and the Polar lands, so far as they have any animal life at all.* These sub-regions, as Mr. Wallace admits, are not so well characterised as might be desired, and their boundaries, and even their number, are therefore open to doubt.

In the Neotropical region the four divisions are—the Andean, comprising Patagonia, the chain of the Andes, and the Pacific Coast up to the Equator; the Central American, with the hot low-lying coasts of Mexico; the Antillean, including all the West Indies except Tobago and Trinidad. The remaining sub-region comprises these two islands, Guayana, Venezuela, and the greater part of New Grenada and Ecuador, those portions of Peru and Bolivia which lie on the eastern slope of the Andes, the vast empire of Brazil, and a part of Paraguay and the States of La Plata. This arrangement has not escaped criticism. It has been pointed out that the last sub-region, superficially very large in proportion to the three others, and greater still in the amount of fruitful land which it contains and in its multitude of animal species, may probably be found less homogeneous than Mr. Wallace supposes. We do not think that the attention of naturalists has been sufficiently directed to a passage in which Sir R. Schomburgk, speaking of the brilliant flora of the mountains of British Guayana, notices the almost total absence of insects. The remark has been made that this statement, if confirmed, agrees ill with certain modern views on the part played by insects in the fructification of plants and on the use of brilliant colouration in flowers. Waterton, whose testimony would have been invaluable, unfortunately paid little attention to insects.†

It is instructive to compare the West Indian islands with the Malay Archipelago. Both these groups are closely similar in soil, climate, and possible productions, but the poverty of the Antilles, whether in the higher or lower forms of animal life, is as remarkable as the wonderful riches of the Eastern islands.

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* We have always been of opinion that the extremely high latitudes would be found completely devoid of a fauna, and we observe with much interest that this view is decidedly confirmed by the results of the late Polar Expedition. The explorers appear to have reached, and even passed, the boundaries of animal life. The bearing of this fact upon the notion of a circumpolar zoological region is obvious.

† Mr. Norman Moore, in his edition of Waterton's "Essays," declares that Schomburgk "has copied whole pages from the 'Wanderings' with no other change than the transformation of an interesting into a heavy style, and, notwithstanding all his obligations to Waterton, he has never once mentioned him in his books with respect."

Turning to the Australian region we find again four sub-regions :—Austro-Malaya, or New Guinea ;* Celebes and the Moluccas ; Australia proper, with Tasmania, New Zealand ; and, lastly, the South Sea Islands. Mr. Wallace doubts, however, whether the Sandwich Islands are not entitled to rank as a fifth sub-region, distinct from the rest of Polynesia, and having affinities which apparently point to a former connection with the western coast of North America. Concerning the Philippines, he is also uncertain whether they should be ranked with the Austro-Malayan sub-region or assigned to the Malayan section of the Oriental region. This being the case it is the more to be regretted that the author omits to furnish an analysis of the insect forms of this interesting group.

The distribution of animals, recent and fossil, in the Australian region is the more important since from its study we may hope to obtain light on such questions as the former existence of a vast equatorial continent of which the Polynesian islands are the mountains, or of an antarctic continent of which New Zealand may have formed an outlying portion, or of a former eastward extension of Australia and its connection with or approximation to New Guinea.

The divisions of the Oriental or Indian region are—the Malayan, comprehending the Sunda Islands, with the peninsula of Malacca, and possibly the Philippines ; the remainder of further India, with the southern coast of China, and probably the islands of Hai-nan and Formosa ; southern Hindustan, with Ceylon ; and northern Hindustan. The position of Hai-nan, and especially of Formosa, is somewhat doubtful. Its fauna has, on the one hand, affinities with that of China (Palæarctic region), and, on the other, with those of the Indo-Chinese, and even the Malayan sections of the Oriental region. Here, again, it is to be regretted that no information has been given concerning the insects of the two islands.*

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* Reports have been spread which if verified would have necessitated the total exclusion of New Guinea from the Australian region. Thus one traveller declared that he had met with the recent dung of a rhinoceros, and had seen the track of buffaloes in the mire. A more competent observer, however, Signor D'Alberti, has shown that the dung was merely that of the *Casuarinus*, and that the tracks had been made by wild swine. New Guinea is remarkable for the beauty of its insects and of its birds. According to Mr. Wallace fully 50 per cent of the latter are brilliantly coloured, whilst in such districts as Malacca and the Valley of the Amazon the proportion is not above 33 per cent.

The Andaman and Nicobar groups have been assigned, the former to the Indo-Chinese and the latter to the Malayan region, an arrangement which we think no judicious zoo-geographer will call in question.

But a much more serious question remains. Certain naturalists, who have made the fauna of India their especial study, hold that its affinities with Africa overbalance those with Malaya. Mr. Crawford maintains that this Ethiopian affinity was more pronounced in the tertiary period than it is at present. It is further urged that as there is easy communication for birds, and even for mammals, between India and Indo-China, or even Malaya, but long tracts of sea and desert between the former and the Ethiopian region, the affinities of India and Africa ought to be estimated at a higher value than if the means of access were equal in each case. Still, as far as the Mammalia are concerned, we question if the affinities of the Indian sub-region for Africa are more pronounced than those for Malaya, or even for the Palæarctic territories. Bears and deer occur in India, but have not been met with in Africa, either living or fossilised. Of the thirty-eight mammalian genera inhabiting India proper, eight are so widely distributed as to give no special clue to the question; "fourteen are exclusively Oriental; five have as much right to be considered Oriental or Ethiopian, extending as they do over the greater part of the Oriental region; two (the hyæna and gazelle) show Palæarctic rather than Ethiopian affinity; seven are Palæarctic and Oriental, but not Ethiopian; and only two (the hunting leopard and the *Mellivora*) are distinctly Ethiopian."

The Ethiopian region, if we include Madagascar, is also divided into four sub-regions, which, however, Mr. Wallace regards as "in some extent provisional." The East-African sub-region includes all the open country south of the Great Desert, and extending eastwards to the Indian Ocean, and southwards to about 20° S. latitude. It has few peculiar forms, and its north-eastern regions are almost as much Palæarctic as Ethiopian; while the fauna of the forests of Mozambique, though on the eastern coast, approaches in character to the western or southern sub-region. The western district includes the mass of forest which lies in the form of a crescent along the Gulf of Guinea, having its northern limits at the River Gambia; extending eastwards to the head-waters of the Nile and the mountains forming the western boundary of the great lakes of Central Africa, and reaching southwards " to that high but marshy forest country in which Livingstone was travelling at the time of his death." Its extreme southern limit may be about 11° S. latitude. The author tells us in a footnote that Dr. Schweinfurth found this region very sharply defined in 4½° N. lat. and 28½° E. long. A sudden change occurs here in the character of the vegetation, and the chimpanzee and the West-African grey parrot first make their appearance. The South-African sub-region, which Mr. Wallace pronounces " the most peculiar and interesting part of Africa," occupies the extreme south of the continent, as far as the Kalahari Desert and the Limpopo River. The more typical portion of the region scarcely extends beyond the boundaries of the Cape Colony and Port Natal.

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* A large amount of our knowledge on the distribution of animals is due to sportsmen, who often do excellent service as regards mammals and birds, but who generally overlook the reptiles, and almost invariably ignore the insects.

There is evidently much to be done before the fauna of Africa can be mapped out with even approximate exactness. Possibly some of its variations may be considered cases of " station " rather than of " habitat." The western district is a luxuriant forest, whilst the eastern is to a great extent elevated table-land, with a vegetation of high grasses, thorny scrub, and here and there patches of forest. It is very intelligible that the animal population of two such districts should show a well-marked difference. We need not wonder, therefore, that the forests of Mozambique should possess a fauna more western in its character, and that the Ethiopian affinities of the Indo-Malayan sub-region should point more to Western than to Eastern Africa.

The Palæarctic region, lastly, is divided into the following four sub-regions:—the European, comprising all the land north of the Pyrenees, the Alps, the Balkan, the Black Sea, and the Caucasus, and west of the valley of the Irish and the Caspian, and including the British Islands, " whose animal productions are so uniformly identical with continental species as to require no special notice." The Mediterranean sub-region includes Spain, Italy, Turkey (European and Asiatic), Persia and Affghanistan up to the banks of the Indus, Northern Arabia, Egypt to the second cataract of the Nile, Northern Africa so as to include the extra-tropical portion of the Sahara and the Azores, Madeiras, Canaries, and even the Cape Verde Islands. It may at first sight seem strange that the northern and southern shores of a deep sea like the Mediterranean should belong to the same sub-region, since deep seas generally mark out a primary division in the faunæ of the lands they separate—a truth which Mr. Wallace has enforced and utilised in his work on the Malay Archipelago. But the Mediterranean, though a very deep sea, did not always form a continuous barrier between Europe and Africa. It was bridged over at Gibraltar, and again at the part between Sicily, Malta, and the African coast, and thus an easy communication between its northern and southern shores was possible. The traveller who proceeds from Spain or Italy, either southwards or eastwards, finds no very marked transition until he has reached the Niger in the one direction or the Indus in the other. The faunæ of the Azores, Madeiras, and Canaries have been carefully examined, and show unmistakable Palæarctic affinities, having been derived at an early period either from South-Western Europe or North-Western Africa. This circumstance speaks against the supposition that these island-groups are the last remains of a former continent (Atlantis), either independent or connected with tropical America. In either of these cases their faunæ would have exhibited a marked distinction from that of Mediterranean Europe.

The third Palæarctic sub-region, the Siberian, occupies the whole of Northern, North-Eastern, and Central Asia as far as the frontiers of the Oriental region. Middle and Northern China and the islands of Japan form the fourth and last sub-region.

Each of the six great regions "has had a history of its own, the main outlines of which we have been able to trace with tolerable certainty."

The question now arises, what are the causes of the distribution of animals as at present existing—a distribution which mere differences of temperature, of moisture, and of the supply of food are far from fully explaining?

Among the agencies which have influenced the migrations and re-migrations of species, a prominent rank belongs to the Glacial epoch—perhaps we might rather say the Glacial epochs. It is important to note that on this subject Mr. Wallace accepts the views of Mr. Belt. This acute geologist holds that glaciation was simultaneous over both hemispheres, and that the amount of water piled up in the form of ice upon the continents so far lowered the depth of the ocean as to lay dry extensive tracts of land, now submerged to the depth of 2000 feet, these low-lying regions becoming the refuge for tropical forms of animal and vegetable life. It is scarcely necessary to add that this hypothesis would never have secured suffrages so exalted if it did not harmonise well with the phenomena of the variation and distribution of species. But there are undoubtedly difficulties in the way. A simultaneous glaciation of the whole earth, preceded or followed by a period like the Miocene, with a luxuriant vegetation penetrating at once almost to either Pole, implies a wonderful oscillation in the total available amount of heat at the earth's surface.

Another important cause has been the fluctuation in the distribution of land and water on the earth's surface. It is true that on this subject exaggerated views have been entertained. Mr. Wallace does not consider it either justifiable or necessary to assume that—at any rate in recent geological periods—the great masses of land and of water have changed their respective positions; but he holds that along the margins of the continents great mutations may have taken place. Northern Asia and the Sahara are probably recent upheavals of shallow sea-bottoms. Borneo, Java, and Sumatra may probably have been united with further India, and again separated; Australia may have very probably extended to the great barrier reef, and the Antilles may have been part and parcel of the American continent. Nay, at a very early period Madagascar may have been connected with Africa, and subsequently with Ceylon and Southern India.

Such changes would follow naturally from the conditions assumed in Mr. Belt's hypothesis of the Glacial period; but Mr. Wallace decidedly opposes the theory of a former direct communication either between Africa and South America or the latter and Australia. The occurrence of certain peculiar forms of life in countries mutually very remote, and their absence in intermediate regions, may, he shows, be explained much more naturally than by the assumption of direct and special connection. Such species may have been once very widely diffused, but being worsted in the great struggle for existence they have been exterminated, save in remote islands and inaccessible districts. Such species it will be found are rarely powerfully armed. As a confirmation of this view, it may be noticed that the strongest and most formidable animals are found either in the large continents or in the islands recently dissevered from them, such as Sumatra or Java. The faunæ of true oceanic islands, on the contrary, are characterised not merely by scantiness, but by the general feebleness and inoffensive character of the species. A very curious fact, to which Mr. Wallace more than once refers, is the evident extinction of many large species of animals since the post-pliocene times, and either just before or shortly after the appearance of man. This phenomenon was apparently simultaneous in Europe and America, and will doubtless be traced in other quarters of the globe when their palæontology has been more fully studied. Among the animals which have thus disappeared are some which seem to have been eminently qualified to "hold their own." Such is the extinct genus *Machairodon*, or sabre-toothed tiger, several fossil species of which, apparently more formidable than any existing cat, have been found in both continents.

Another very important conclusion is, that the theory of the independent origin and development of animal life in a number of distinct points can no longer be upheld. The great northern continents appear to have been the seat and birthplace of all the higher forms of life, whilst certain strange creatures—such as the gigantic fossil Edentata—seem to have originated in the south and to have gradually spread northwards.

In one respect we must own ourselves disappointed with Mr. Wallace's book, although the author, in his Address delivered before the Biological Section of the British Association at Glasgow, has done very much to supply the deficiency. We had expected that the work would have contained a summary of facts, and possibly some interesting generalisations on the influence of locality on the colour, the size, and the form of animal species. Every naturalist —save such, if they deserve the name, who confine their researches to books—knows that the fauna of each country has a peculiar general physiognomy, more or less pronounced. These peculiarities are sometimes difficult to express in words, and may escape any but the most patient observer, but in other instances they are open and palpable. Thus, according to Mr. Goodman, there appear in the birds of the Azores modifications all tending towards a more sombre plumage, and a greater strength of feet, legs, and bill. Mr. Blanford finds that Persian specimens are, on the average, paler in colour than their nearest allies in Europe. Mr. Wallace, in his Glasgow Address, remarks that it is "in islands we find some of the most striking examples of the influence of locality on colour, generally in the direction of paler, but sometimes of darker and more brilliant hues, and often accompanied by an unusual increase of size." He then shows how the butterflies of certain genera, such as the *Euplæas*, are in the larger islands dark-coloured, whilst in Banda, Ké, and Matabello there are three species, not nearly related to each other, "all broadly banded or suffused with white." "The small island of Amboyna produces larger-sized butterflies than any of the larger islands which surround it. This is the case with at least a dozen butterflies belonging to many distinct genera."

Corresponding cases of paleness are found in Fiji, in the Andamans, and in Jamaica.

In Celebes, as Mr. Wallace has shown in an earlier work,* instead of any modification of colour, there is "a peculiar form of wing and a much larger size running through a whole series of distinct butterflies."

The Philippine Islands seem to have the property of developing intense metallic lustre, both in butterflies and beetles. Thus the hind wings of *Ornithoptera Magellanus* "glow with an intense opaline lustre not found in any other species of the entire group, and *Adolias calliphorus* is larger and of more brilliant metallic colouring than any other species in the Archipelago. In these islands, also, we find the extensive and wonderful genus of weevils, *Pachyrhynchus*, which in their brilliant metallic colouring surpass anything found in the whole eastern hemisphere, if not in the whole world."

Continental districts likewise have their peculiarities of colouration. Thus two unrelated groups of butterflies from tropical Africa "are characterised by a prevailing blue-green colour not found in any other continent." Similarly, in South America, "nine very distinct genera are implicated in parallel changes, groups of three or four of them appearing in the same livery in one district, while in an adjoining district most or all of them undergo a simultaneous change of colouration or marking."

These local peculiarities may perhaps be overlooked because they occur in insects, but Mr. Wallace very aptly asks—What should we think if similar phenomena were to be traced amongst large mammals?

In birds, however, local characteristics of a corresponding nature occur, as the author proves by reference to the avifaunæ of the West Indies, the Andamans, the Philippines, Celebes, Timor, Flores, and Lord Howe's Island. In New Guinea, Australia, Madagascar, and the Mascarenes we have black parrots and pigeons—a curious instance of the phenomenon known as melanism.

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* Contributions to the Theory of Natural Selection, pp. 168—173.

It has been maintained that, in the Nearctic region at least, the Mammalia increase in size with the latitude and altitude of their birthplace. This view is disputed by Mr. Allen, who declares that it does not agree with the development of the American Carnivora. Indeed as regards altitude, and consequently rarefaction of the ambient medium, the very opposite law has been proposed. The largest animals are now found in the denser medium, water; many of the next largest species—such as the elephants, hippopotamus, &c.—inhabit river-marshes and deltas almost on a level with the sea. There is also good reason for supposing that the atmosphere in the epochs which produced the gigantic extinct animals must have been denser than it is at present. Mr. Allen considers that the largest individuals of every species, and the largest, best-developed, and most typical species of every group, will be found near its centre of distribution.

It has also been suggested, at least as regards insects, that in all regions dark-coloured species are characteristic of woods, whilst white or light-coloured forms occur in the open plains. This law holds good in some well-known genera of butterflies, such as the "whites" as compared with the forest-loving *Hipparchias*, *Erebias*, &c.; but the deeply-coloured *Vanessas* inhabit the open country. Among Coleoptera we find the dung-beetles, mainly black in colour, abundant in the open country, like the ruminants on whose excrement they prey. The common ground-beetles, also (Harpalidæ, &c.), are chiefly sombre in colour, and certainly show no exclusive preference for woods. On the other hand, the chafers (Melolonthidæ) and the Buprestids frequent the woods, and yet display some of the most striking instances of pale colouration to be found among the entire tribe of Coleoptera.

For further details on the influence of locality we must refer the reader to Mr. Wallace's most instructive Address, but we cannot help expressing our regret that he has not introduced the consideration of this subject into his *magnum opus*.

We may rest assured that the peculiarities' to which we have so briefly referred point to causes of variation at work other than protective mimicry or than sexual selection. This is evidently the opinion of Mr. Wallace, who insists that "one of these causes is an influence depending strictly on locality, whose nature we cannot yet understand, but whose effects are everywhere to be seen when carefully searched for. If it be asked why so little attention is given to this and to other interesting problems connected with the distribution of organic life, one important cause has been pointed out by Mr. Wallace—the total want of a museum geographically arranged. Our museums, more or less complete, are arranged morphologically. Birds, reptiles, insects, &c., which approximate in their structure are placed together, quite irrespective of the locality from which they have been obtained. Such collections are obviously indispensable, and all we recommend concerning them is that they should be made much more complete and more accessible than is now the case, and should invariably be placed in some central position, and not in a remote, even though fashionable, suburb. But along with such we want also a museum geographically arranged, where we may see, *e.g.*, in one hall the fauna of India, in another that of New Guinea, of Australia, of Madagascar, or of the Cape. Who can doubt that if such collections were accessible, relations, similarities, contrasts would strike us which escape unnoticed when the species of the whole world are placed side by side. It would likewise be instructive to exhibit the species of every country in juxtaposition with their nearest allies or representatives in other countries, and to show specimens of widely-distributed species from the centre and the extremes of its range. Surely if any nation can produce such an institution it ought to be England; yet hitherto we have little even pointing in this direction save the collections of "British" birds and insects, which have been multiplied both by public institutions and by private collectors, and which are the less instructive because Britain is not a definite zoological district, but merely an impoverished portion of the north-western Palæarctic region. Is it too much to hope that the great Colonial Museum which looms in the future, and which by the special favour of all good powers is to be placed on the Thames Embankment, may include a department of the kind desired?

We must now, however, take our leave of Mr. Wallace and of his truly magnificent contribution to Natural History. If we cannot pronounce the work as in all respects perfect—if we here and there entertain a doubt or desire fuller information—the cause lies not in any shortcoming on the part of the author, but in the extent and the complexity of the subject and in the limited state of our present knowledge. The plan which he has traced with so masterly a hand will doubtless be elaborated in its details, and may perhaps here and there be somewhat modified. But we shall all feel that Alfred Russel Wallace is the architect whose designs we are carrying out.

V. ON THE LOESS OF THE RHINE AND THE DANUBE.

By Thomas Belt, F.G.S.

THE sides of the valleys of the Rhine and the Danube, as well as those of their tributaries, are covered with a yellowish grey calcareous clay up to a height of several hundred feet above the rivers. This clay is often of great depth, is unstratified, and contains land and fresh-water shells scattered throughout it. It also contains the bones and the stone implements of palæolithic man, and the remains of the mammoth, the woolly rhinoceros, and other Mammalia. This clay has been named the Loess, and respecting its origin and its relation to the excavation of the great valleys there is much difference of opinion.

Sir Charles Lyell, in commencing an able summary of the facts respecting the distribution of this deposit, states that skilful geologists peculiarly well acquainted with the physical geography of Europe have styled the loess the most difficult geological problem.* It is certainly curious to contrast the ease with which some philosophers map out the world in former geological periods, showing with confidence which areas have been beneath the sea and which remained dry land, with the difficulty that is experienced in getting any idea of what was the condition of the continents during the formation of the latest deposits, after man had penetrated into Europe, and all the species of animals and plants now living had come into existence. And yet, until we understand the youngest of the geological formations, it is almost idle to speculate on the more ancient conditions of the earth's surface. Whilst, for instance, it is held by many that much of northern Europe and America was depressed below the level of the sea in post-tertiary times, and rose again to its former and present level, yet that the deposits formed beneath the waters of that ocean contain no marine organisms over thousands of square miles, what confidence can we feel in those theories that map out the continents in older geological periods according to the presence or absence of the remains of the inhabitants of the sea. The uncertainty felt respecting the latest formations fully justifies the large share of attention now paid to them and the efforts that are being made by many minds to bring the facts within the range of some consistent theory. This is not a case where the facts are unknown. No other formation has been so much studied as the quaternary. What is needed is a consistent theory of origin which shall violate no natural laws, be in harmony with the older geological record, ask for the aid of no unknown force, and in which every fact shall fall into its natural place and find its full explanation.

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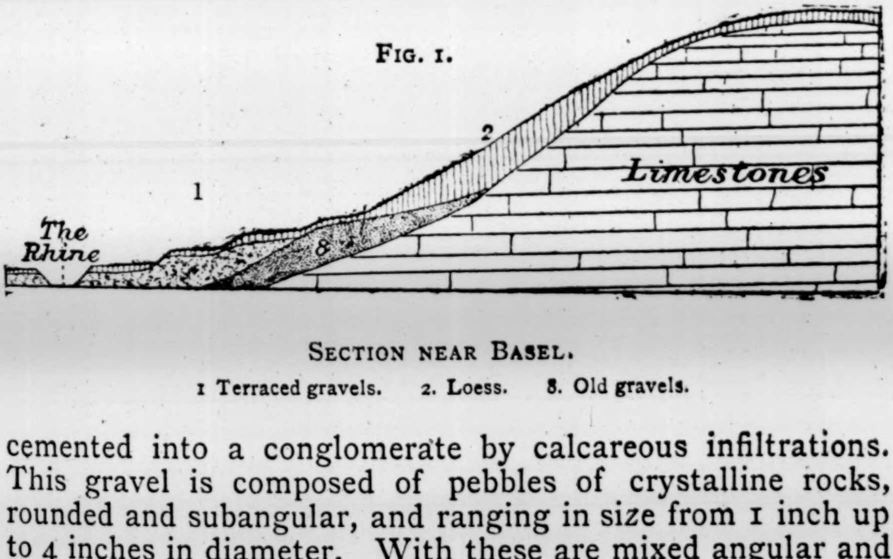
* Antiquity of Man, 4th edition, p. 372.

Impressed with the importance of the question, I have during the last twelve years endeavoured to make myself thoroughly acquainted with the facts, and have had unusual facilities for studying them in both hemispheres. When known, they appear to marshal themselves into a certain order, and to suggest a theory of origin that I have supported in the pages of this journal and elsewhere, but for which I have not yet obtained much consideration from geologists. My business occupations not leaving me sufficient time to prepare a general treatise, in which the various questions might be fully worked out and their relations to each other shown, I have been obliged to present my theory in short essays on distinct groups of facts, and in the present paper I propose to take into consideration the difficult question of the distribution of the loess, and to show that it is explained by the theory that at the culmination of the Glacial period the ice principally occupied the ocean depressions, and blocked up the drainage of the continents as far as it extended.

The loess, in the valleys of the Rhine, the Danube, and their tributaries, is found up to a great height above the present rivers. That of the Rhine is well illustrated around Basel, and the following section (Fig. 1) is one of many that may be seen in that neighbourhood. It is taken on the right bank, where a low range of vine-clad hills comes down nearly to the river, about 2 miles east of the city. Quarries of limestone are worked in the hill-side, and fine sections of the loess exposed. It is here a grey calcareous clay, of such a firm consistency that the workmen have excavated a chamber in it to keep their tools in; and this is its general character wherever it is seen. Land shells are scattered throughout it, and I found them up to a height of about 1110 feet above the sea, which was as high as there were good sections of it exposed; but above this I traced it to the top of the hill, or to a height of about 1470 feet above the sea and 660 feet above the river. I took my altitudes by means of an aneroid barometer, calculating from the known height of the river at the bridge at Basel (803 feet), and though not absolutely correct they may be relied on to within a few feet, as I returned to my starting-point and found that only a small change had occurred in the barometric pressure in the meantime. Besides the land shells, small angular pieces of the local limestones and calcareous concretions are distributed throughout the loess. At the base of the hill it rests on a bed of gravel that has been cemented into a conglomerate by calcareous infiltrations. This gravel is composed of pebbles of crystalline rocks, rounded and subangular, and ranging in size from 1 inch up to 4 inches in diameter. With these are mixed angular and subangular fragments of the local limestones, of greater size. The highest point at which I noticed this old gravel-bed was 960 feet above the sea. It rests against the old valley bank, and the loess is distinctly superimposed upon it, and contains in its lowest part many pebbles derived from it. The importance of this fact will be recognised when I come to the discussion of the question of the origin of the loess.

FIG. 1.



SECTION NEAR BASEL.

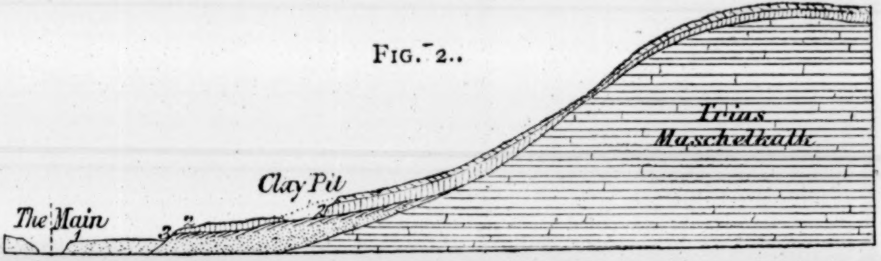
1 Terraced gravels. 2. Loess. 8. Old gravels.

Between the base of the hill and the river there are three terraces of gravel capped with clay, at the heights respectively of 923, 860, and 830 feet above the sea, the lowest being about 25 feet above the river. I think that these may have been of more recent origin than the loess, and that the clay with which they are covered may have been washed down from the hills above them.

The loess may be traced all round the flanks of the hills that surround the great plain between Basel and Bingen, and also on the hills that in some places rise up like islands in its midst. Thus Sir Charles Lyell notes that it covers, up to a height of 1600 feet above the sea, the Kaiserstuhl, —a volcanic hill which stands in the middle of the great valley of the Rhine, near Freiberg, in the Brisgau. It is also spread over the volcanic hills of the Lower Eifel. Dr. Samuel Hibbert, in his "History of the Extinct Volcanoes of the Basin of Neuwied on the Lower Rhine," published in 1832, gave many instances of the intercalation of the loess with beds of volcanic ashes near Andernach. Much white pumice is spread over the loess, and it seems to be well ascertained that both during its deposit and afterwards the volcanoes were active. The veteran geologist, Herr Henry von Dechen, has mapped out these deposits, and described them in his "Geognostischer Fichrer zu dem Laacher." He has informed me that one of the highest patches of the loess on the volcanic hills lies south of Andernach, near the hill of Korrets, at an altitude of about 620 feet. Dr. Hibbert appears to have found it higher, for he states that on the Mahlsberg the loess attains an elevation of 800 feet above the sea, or 600 feet above the river, and is sometimes 60 feet thick. With regard to the pumice that is abundantly spread out on the top of the loess between Andernach and Coblentz, and which Herr von Dechen informed me indicated that the volcanoes were active at the time of the close of the deposition of the loess, it may be worth remarking that the *Challenger* explorations have shown that great quantities of pumice are spread over the bed of the ocean, proving it to be a common product of submarine volcanoes; so that it is quite in accordance with our knowledge of its production to suppose that the volcanoes of the Eifel at the time of its eruption may have been submerged beneath the waters from which the loess was deposited.

The loess extends up the valleys of the tributaries of the Rhine. The basin of the Neckar is, according to Sir Charles Lyell, filled with it. It is of great thickness, and at Canstadt, near Stuttgart, overlies a bed of gravel, and contains bones of the mammoth and the woolly rhinoceros. There are thick deposits of loess in the valley of the Main. I had the great advantage of examining it near Wurzberg, in the company of the celebrated geologist Dr. Sandberger. It has been principally preserved, from the effects of great floods that appear to have swept down the valley, in bays and recesses, and at these points bones of the mammoth and its associates have been found. I took the following section (Fig. 2) at Blosenberg, near Heidingsfeld, where the loess is extensively dug for brick-making. At the clay-pit the lowest bed seen is one of subangular quartzose gravel, with pebbles of crystalline rocks from the mountains at the sources of the Main. This is covered with clean false-bedded sands, containing lines of small angular and subangular pebbles, mostly of quartz. The sands are covered by loess, which at the base is sandy and a little stratified, but soon graduates upwards into unstratified calcareous loess with vertical joints. It is divided into two beds by a clear line of division, the upper one of which is of a lighter colour than the lower. In another section, in a small valley to the south of Marianberg, the division line is irregular, and is strongly marked by the occurrence along it of angular fragments of limestone.

Fig. 2.



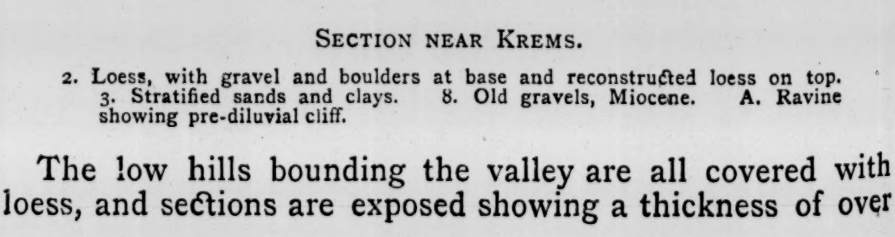
Section near Wurzburg.

1. Alluvial plain. 2. Loess, with rubble and reconstructed loess on top.
3. Gravels and false-bedded sands underlying loess.

At Blosenberg the loess conforms to the configuration of the ground, and in all the sections I saw, the different divisions were inclined with the slope of the hill, and had not been deposited in horizontal strata that had been afterwards denuded, but rather as a mantle on the slopes of a pre-existing valley. Shells of land mollusks are very abundant at Blosenberg, particularly on the slopes of the hill, where there are continuous sections by the sides of the deeply-cut paths that lead up through the vineyards. They occur here mostly in lines that give a sort of stratification to the loess. These lines of shells rise with the slope of the hill. I traced them up to a height of about 670 feet above the sea, and the loess up to 714 feet. From thence upwards the steep slope is mostly covered with limestone *débris* until we get to the summit, which is again covered with loess. On the northern slope of the hill I found a thin layer of characteristic loess, beneath local *débris*, up to a height of 880 feet above the sea. On the hill-tops it occurs to 1100 feet or more, and Dr. Sandberger informed me that it is sometimes thick on the hill-tops, and contains the characteristic loess shells, though not so abundantly as lower down in the valley. I am inclined to think that it was originally deposited continuously over the slope up to the tops of the hills, but has since been removed by denudation, as the places where it is absent are just those where it would be most likely to be washed off, and it appears to be present wherever the ground becomes flatter.

In the basin of the Danube the loess is more generally and thickly spread out than even in the watershed of the Rhine. Prof. Edward Suess has kindly furnished me with much information respecting its distribution. It is found up to a height of at least 1300 feet above the sea in the upper part of the valley of the Danube. Where the river issues from the narrow gorges that it has cut through the crystalline rocks that range southwards from Bohemia, and again through the northern prolongation of the Alps above Vienna, the loess is heaped up as if deposited at the head of a lake, and just as the mud of the Rhone is now being deposited at the upper end of the Lake of Geneva. I visited Krems, where the valley—greatly contracted above where the river passes through gneiss and other crystalline rocks—widens out into a great plain, and there I found, as Prof. Suess had before informed me, the loess heaped up in a cone around the expanding mouth of the gorge. A little below the town of Krems, on the left bank of the Danube, I got the following section:—

FIG. 3.



SECTION NEAR KREMS.

2. Loess, with gravel and boulders at base and reconstructed loess on top. 3. Stratified sands and clays. 8. Old gravels, Miocene. A. Ravine showing pre-diluvial cliff.

The low hills bounding the valley are all covered with loess, and sections are exposed showing a thickness of over

60 feet without its base being seen. Many bones of the mammoth have been found in it, and it is related that when in the thirty years' war the Swedes were besieging Krems they found in one of their trenches the skeleton of a monstrous animal, and that besiegers and besieged ceased from their warfare for a time to gaze on the huge teeth of the giant that had been dug up.* Some bones were shown to me at the Imperial Museum, by Prof. Fuchs, which bore cuts and indentations apparently produced by a cutting instrument, and which are supposed to have been made by man. There is no reason to question this inference, as a few palæolithic implements have also been found; and that man was a contemporary of the mammoth is now fully established.

In ascending one of the many roads leading up the vine-covered slopes I found deep sections of the loess exposed, and it often rises like a wall on each side to a height of from 20 to 40 feet. The roads appear to have been purposely cut down deep, to allow not only wine-cellars but dwelling-houses to be excavated in them. The loess at Krems is compact, and almost like chalk in its homogeneous consistency, without the joints and fissures of the latter. It contains patches and seams of gravel, and pebbles of quartz are irregularly scattered throughout it. It is unstratified, but occasionally lines of division are seen separating portions of slightly different colour and composition. At its base I saw much gravel, and some boulders resting on the denuded top of stratified sands and clays. At one point I found that it had been banked up against a pre-diluvial precipice, as shown at A in Fig. 3.

In some parts patches of miocene gravels lie below the stratified sands or clays, and in these the remains of *Mastodon longirostris* have been found. Prof. Suess informed me that he had determined that these gravels were heaped up in Miocene times in the same parts of the valley where in the Quaternary times the loess was most thickly deposited, and he thinks that at that early period the valley had assumed in a great measure its present configuration.

The loess is by no means confined to the valleys of the great rivers and their tributaries. Sir Charles Lyell† and Mr. Godwin-Austen‡ both follow the Belgian geologists in identifying it with the "Limon de Hisbaya," which covers much of Belgium, enveloping Hainault, Brabant, and Limburg like a mantle. It extends into France, and covers the high plains between the rivers, and is most likely the same as the upland loams of the valleys of the Seine and the Somme, which occupy positions often independent of the present lines of drainage. It is even probable that it once covered the south-east of England, but has since been denuded, as Prof. Morris has shown to me some of its characteristic shells which he had gathered in patches of loess, preserved in fissures of the chalk, near Maidstone.

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* I am indebted to Prof. Edward Suess for this anecdote.

† Antiquity of Man, p. 329.

‡ Quarterly Journ. Geol. Soc., vol. xxii., p, 251.

There is also much evidence tending to prove that the loess is the equivalent in the river valleys of the northern drift that covers so much of Northern and Central Europe. Thus in ascending, from Vienna, the valley of the March, I found all the flanks of the hills covered with a loess like clay, which as I travelled northward changed to a redder colour. This clay covered the low watershed between the Danube and the Oder, to a depth of 20 feet in some places. The watershed is only about 1000 feet above the sea, and the clay covers it and follows along the flanks of the hills on either side. On the northern flanks of the range, Scandinavian blocks, that must have been carried from the far north, are found up to heights of over 1200 feet. These occur all along the northern side of the Carpathians. The diluvial clay extends much higher. According to Prof. Stur it fills the valleys on the north slope, and I have always found it extending to much higher levels than the northern blocks. This is what we might expect, for the icebergs that brought the blocks from the far north must have required a considerable depth of water to float them.

In 1875, at Wolochisk, in the province of Volhynia, near the Russian frontier, whilst detained for the examination of luggage and passports, I found, in the clay that had been thrown out of a well, shells of *Succinea oblonga* and *Pupa muscorum*, two mollusks that have left their shells in the loess almost everywhere where it occurs. This was not in a valley, but on the top of the flat plateau that forms the steppes of Southern Russia. I was much impressed with the occurrence of these characteristic loess shells at this place, as no higher ground overlooks the plain, and the clay may be followed continuously for hundreds of miles, and to the north contains transported Scandinavian rocks. On my return from a visit to Southern Russia, during the present year, I determined to examine this clay, which is "the diluvium" of the Russian geologists, nearer to the mountains, and for that purpose stayed twelve hours at Podwolochisk, in Galicia. I was greatly gratified by finding loess shells in abundance, and thus adding another link to the chain of evidence that connects the loess with the northern drift. This northern drift and the clay that accompanies it is the Upper Boulder clay of the east of England; and in 1864 Mr. Searles-Wood, jun., was led—simply from the description of the loess of Belgium—to suggest the identity of the latter with the Upper Glacial clay.*

Prof. Suess has found, in the basin of the Danube, some important evidence pointing to the Glacial age of the loess. He showed me pieces of green Hornblendic rock that had been broken from large erratic blocks found in the loess near Vienna. This rock is not known to occur in place nearer than the Wechsel, at the Styrian frontier, and he thinks it must have been carried on and dropped from ice floating over the water from which the loess was deposited. At Wurflach there is the terminal moraine of a glacier that once came down from the Schneberg. This moraine contains many scratched and polished blocks of limestone. Many of these have been carried across the Steinfeldt and deposited on the flanks of the range of hills near Pitten, where the local rocks are mica-schists. They lie at a height of about 1100 feet from the sea, and at a distance of 9 miles from the terminal moraine at Wurflach, which is the nearest point from which they can have been brought. They were noticed by M. Morlot, who did not know, however, from whence they had been derived. Prof. Suess traced them to their source, and thinks they must have been carried by floating ice.

The animal remains found in and below the loess also indicate its Glacial age. I have spoken of the gravels that underlie it and belong to an earlier period, in the basin of the Rhine. Some of these gravels appear to be of the same age as the Cromer Forest beds and the Durnten lignites, being characterised by the presence of *Rhinoceros etruscus*, Falc., which has been identified by M. Lartet with the *R. Merkii* of the German and Swiss geologists. The deposits containing this characteristic mammal are well developed at Mosbach, near Biebrich, in the valley of the Main; they consist of sands and gravels overlaid by the loess. Besides the *Rhinoceros etruscus* they contain bones of *Elephas antiquus*, *E. primigenius*, *Cervus tarandus*, *C. megaceros*, and *Hippopotamus major*—a curious mixture of northern species with those that are supposed to have had a more southern range. Probably the southern mammals came northward in summer and retreated to the south in winter. This explanation of the mingling of the remains of northern and southern mammals in the same deposit was first given by Sir Charles Lyell, and has been since advocated with great ability by Mr. Boyd Dawkins. That this is correct—and not that we have here the mixture of the remains of animals belonging to two periods, one warm and the other cold, as urged by Mr. James Geikie—is evidenced by the land and fresh-water shells that accompany the remains of the Mammalia. These could not, like the southern mammals, retreat to the south on the approach of winter, and they are all of a northern character, fitted to withstand great cold. Dr. Sandberger informs us that many of them still live in the higher parts of the Main valley above Bamberg. Others, such as *Valvata naticina* and *Hyalina viridula,* are only now found in the north and north-east of Europe. *Pupa columella* is still living in the north of Russia, in Lapland, and on the Gemmi. *Patula solaria* is now found on the Eastern Alps and the Silesian hills, and others attain their southern limits at Frankfort.* From a consideration of these facts Dr. Sandberger comes to the conclusion that the climate must have been much colder than at present. It would appear to have been so if we take the evidence of the shells only, but when we add that of the mixture of the remains of northern and southern mammals we are rather led to the opinion held by Sir Charles Lyell and Mr. Boyd Dawkins,† that the winters were colder and the summers perhaps warmer than now; that, in fact, the climate was more continental, in consequence of the western coast of Europe having then embraced the whole of the British Isles and other lands now submerged below the sea. If, as has been advocated by Mr. James Geikie, the mingling together of the bones of the northern and southern mammals was not due to the range of their summer and winter migrations overlapping, but to the remains belonging to widely different periods having been afterwards mixed together,‡ it is inexplicable that mollusks denoting a warmer climate should not have accompanied the southern mammals in the valley of the Main. On the other hand, such a fact is fully explained if the winters were colder and the summers warmer, for the slow crawling mollusks could not retreat southward on the approach of winter along with the southern mammals, and only those that could withstand the severe winters would survive.

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* Ann. of Nat. Hist., vol. xiii., p. 185.

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* Geological Magazine, 1874, p. 220.

† *See* Cave Hunting, p. 397.

‡ Great Ice Age, p. 467.

The Dürnten lignites, which, like the Cromer Forest bed and the Mosbach beds, are characterised and have their geological horizon fixed by the presence of *Elephas antiquus* and *Rhinoceros etruscus*, have been shown by Prof. Heer—from a study of the flora preserved in them—to have certainly been formed in as cold and possibly a colder climate than the present. I may remark that Prof. Heer himself correlates the lignites with the Mosbach gravels and the Cromer pre-glacial beds;* and if the arguments that have been used to prove, from the existence of the former, that there have been interglacial warm periods, are of any weight, which I do not admit, then the glacial period that is supposed to have prevailed before the formation of the Dürnten lignites is one of which we have no traces in the British Islands, as it belongs to an epoch anterior to the growth of the Cromer Forest, which is older than any of our English glacial deposits.

Resting on the Mosbach sands and gravels, as well as on gravels of more recent age, and everywhere distinctly superimposed upon them, lies the loess. We have seen that in the gravels of more ancient date the climate was probably colder, or at any rate the winters were more severe than now. The fauna of the loess indicates a still colder climate. The hippopotamus, the southern elephant, and the southern rhinoceros are no more found. The mammoth abounds, and is now accompanied by the woolly rhinoceros. The reindeer and the Canadian elk are much more abundant than in the earlier period. Fresh-water shells occur but rarely, and belong to species such as *Lymnæus truncatalus* (the only one found near Wurzberg), which probably lived not in the water from which the loess was deposited, but in marshy spots above it. River shells are unknown: I have never seen even a fragment of one in the loess. The most abundant land shells are either those now ranging far to the north, or which, from their wide distribution, must be able to accommodate themselves to extremely varying circumstances. Thus the most characteristic of all the shells—the *Succinea oblonga*, which I have gathered in the loess of the Rhine, the Main, the Danube, and the Steppes of Southern Russia, and which Prof. Morris finds in the patches of loess preserved in fissures of the chalk near Maidstone—is, though widely ranging, essentially a northern species, being at present rare in the basin of the Rhine, but abounding much farther north, in Scandinavia and Northern Russia. The little *Pupa muscorum*, which nearly everywhere accompanies the last mollusk in the loess, ranges throughout Europe from Iceland and Lapland to the Mediterranean. The minute *Helix pulchella*, which I have also found very generally distributed in the loess, has a still wider range, being found from Siberia to Corsica, throughout Canada, and even in the Azores. *Helix hispida*, a common loess shell, and *H. sericea*, which replaces it near Wurzberg, range from Siberia to the Mediterranean.

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* The Primæval World of Switzerland (Eng. ed.), pp. 171 and 176.

The fauna of the loess is the same in the basins of the Danube and the Rhine, and in Southern Russia; it belongs entirely to the period of the mammoth and the woolly rhinoceros. Neither the highest nor the lowest patches of it contain the remains of any other fauna. It is one fitted to endure a cold climate. It is the culmination of a series showing a gradual refrigeration of the northern hemisphere from early tertiary times. The tropical forms of the Eocene strata are succeeded by the semi-tropical ones of the Miocene: these by the more temperate species of the Pliocene; then comes in the fauna of the Mosbach gravels, the Dürnten lignites, the Cromer Forest and the oldest Thames brick-earths, when palæolithic man first appears on the scene in Northern Europe, and when the mean temperature was probably about the same as now, though the winters were colder and the summers warmer. Then, distinctly superimposed upon the Mosbach sands and gravels, comes the last fauna of all before the present,—that which is found in the newer gravels and the loess resting on them,—the fauna of the mammoth and the woolly rhinoceros, along with which we find the latest remains of palæolithic man in Northern Europe. That that fauna belongs to the Glacial period is evidenced not only by the fact that it is composed of species best able to withstand extreme cold, but that in the valley of the Danube large boulders occur in the loess that must have been transported on icebergs whilst it was being deposited.

No more arctic fauna is known in the basins of the Danube and the Rhine than that of the loess. Above it, as elsewhere, at this stage, there is a great break in the succession of life; the mammoth, the woolly rhinoceros, and palæolithic man disappear; and after that interruption the next forms that appear are those of the recent period. Neolithic man now takes possession of the land, and with him comes a number of animals that still remain with us, with the exception of the Irish elk in Europe and the mastodon in America, which have perished since the commencement of neolithic times.

I have now given all the most important facts that I know of respecting the position and distribution of the loess, and we may proceed to consider the theories that have been proposed to account for its origin. First of all we shall have to determine, as well as we may from the facts before us, whether the loess was deposited in previously formed valleys which were as deep or deeper than they are now, or that it was laid down in the process of the excavation of the valleys. The former view was that held by Sir Charles Lyell, who had studied the loess both of the Rhine and the Danube, and who in his "Antiquity of Man" has given —with the logical force and clearness that characterise all his writings—a most able summary of the facts that influenced his judgment. These are, briefly, that the loess encircles some of the modern cones of loose pumice and ashes of the Lower Eifel; that it rests on gravel near the bottoms of the present valleys; and that there is evidence that before it was deposited there existed steep slopes, as between Darmstadt and Heidelberg, where loess 200 feet thick and reaching up to 800 feet above the river is banked up against an old valley slope composed of granitic rocks.

Prof. Ramsay, on the contrary, favours the opinion that the loess of the Rhine has been deposited by the river, which, as it gradually lowered the level of the plain, left its finer *detritus* at various heights above it.* The memoir in which this opinion is expressed is an essay to prove that the valley of the Rhine has been excavated by the river,— an opinion not likely to be disputed by any geologist. The question of the deposition of the loess is only incidentally mentioned, and no explanation is offered of the many facts advanced by Dr. Hibbert, Sir Charles Lyell, and others, to prove that before such deposition commenced the valley had been excavated to at least its present depth. The authority of the Director of the Geological Survey of England is so great that it is necessary to insist here upon the cogency of the arguments that have been urged against the theory he espouses, and it is a pity that he has not met them or even alluded to them in his memoir. We have already seen that the loess rests on previously deposited beds of gravel. At Wurzberg this gravel lies at only a slight elevation above the level of the river, and Dr. Sandberger informed me that it goes down to at least 50 feet below it. Here, then, the valley must have been excavated below its present depth to allow of the deposition of the gravel which preceded that of the loess. Near Basel, as I have shown in Fig. 1, the older gravel rests directly against the steep slopes of the rocks bounding the valley, proving again that the latter had been excavated long before the deposition of the loess commenced. At Mosbach the gravels, containing an older fauna than that of the loess, though above the level of the river, are yet several hundred feet below the altitude the loess attains to, and that the latter has all been deposited since, is proved by it everywhere containing a younger fauna. The Mosbach gravels belong to the time of *Rhinoceros etruscus*, the loess to that of *R. tichorinus*.

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* Quart. Journ. Geol. Soc., vol. xxx., p. 89.

These facts are very strongly in favour of the pre-diluvial origin of the great valleys, and there are others still more difficult to explain on any other theory. As already mentioned, the loess extends far up the flanks of the recent volcanic cones of the Lower Eifel, and at Mahlsberg attains an elevation of 600 feet above the Rhine. The most ardent advocate of the theory that the loess has been left by the river whilst it ran at higher levels will not, I think, suggest that the volcanic cones have been carved out by it, and yet excepting on that supposition their theory falls to the ground. Another most serious objection to it is that it requires that the whole of the great valleys of the Danube and the Rhine have been excavated in one comparatively short period, namely, that of the mammoth and woolly rhinoceros. The upholders of the theory may say—"Not excavated, but re-excavated through the gravels and clays that were deposited in them in Miocene times;" but the loess extends far higher up the sides of the valleys than we have any evidence that the Miocene deposits ever reached to.

In the whole of the long Pliocene period it would appear that the land was above the level of the ocean, and we are to suppose, on the theory against which I am contending, that the rivers did not then wear down their channels, but waited for the mammoth period before they commenced doing so. Both in the valleys of the Danube and the Rhine the Pliocene epoch is unrepresented by fossil remains. I asked Prof. Suess what was the meaning of this great hiatus? and he replied that during Pliocene times the rivers were re-excavating their channels through the Miocene accumulations, and that periods of destruction are not periods of deposition in the same areas. At first, the new river channels would probably be cut out with precipitous sides, and it was not until these sides had been sloped down by the action of rain that the loess was deposited.

Adding to the arguments already urged the fact that nowhere in the loess have any river shells been found, whilst they abound in the beds of all our large rivers, we have a case ~~so~~ strong against the theory, that, until its upholders offer some explanation of the facts arrayed against them, we may fairly refuse to receive it. And if we are forced to conclude that the loess has not been deposited by the rivers, and that the excavation of the valleys took place at an earlier time, we must believe that in some way or other these valleys were again filled with water up to the height to which the loess reaches. This is the conclusion of the Austrian geologists with regard to the valley of the Danube, and that of Sir Charles Lyell with regard to that of the Rhine also; and the next question we have to consider is whether the waters were pounded up through oscillations of the surface of the land, or in some other way.

The valleys of the Danube and the Rhine were in existence in early tertiary times, and we find two great accumulations of fossiliferous strata in them,—one miocene, the other post-tertiary. Both appear to be due, not to deposits from rivers wearing down their channels, but to interruptions to their drainage so that their channels were raised. The first of these interruptions is known to be due to the great volcanic disturbances during which the Alps were partly elevated; the second I hope to show was caused by the ice of the Glacial period blocking up the drainage of the continent.

I have already alluded to the transported boulders in the loess, and to its fauna, indicating its glacial age. Along the northern flanks of the Carpathians, the Scandinavian drift rises to heights of from 1000 to 1200 feet; considerably higher than the low passes that lead into the basin of the Danube. General Helmerson has done me the honour to inform me that the transported northern blocks in Russia also range up to heights of from 1000 to 1200 feet above the sea. As the Russian diluvial clay can be traced continuously up to and around the Carpathians into the basin of the Danube, we may reasonably conclude that the water over which the northern drift was floated was the same as that in which the loess was deposited.

But it will be urged that "the northern drift is a marine deposit." It is so stated in most works on geology; but excepting around the southern border of the Baltic, and just so far as, and no farther than, the Scandinavian glaciers reached and carried up fragmentary shells from the arms of the sea they had crossed, the northern drift does not contain sea shells or any other marine organism. For thousands of square miles, south of the irregular line I have indicated, up to and around the Carpathians, the northern drift is spread out, and not a trace of marine life—not even a diatom—has been recorded from it, whilst at its base, between the Oder and the Elbe, fresh-water shells abound. To believe that Europe gradually sank down below the level of the sea until the latter had its shore line more than 1000 feet up the flanks of the mountains, and that it rose again without the sea leaving behind it any traces of life excepting fresh-water shells, is such an extreme hypothesis, and so contrary to all we know respecting the composition of existing sea-bottoms, that it is probable that its present acceptance is simply a survival from the time when there was no other way of explaining the existence of water up to such heights.

The usual explanation of the facts of the Glacial period is one continued appeal to the hypothesis of great oscillations of the earth's surface at that time. It may well be questioned whether geologists have not been too ready to call in the aid of these movements. There is much evidence to show that vast continental areas were never below the sea-level from the close of the Palæozoic period up to the end of the Tertiary period. Yet after this stability of surface over such an immense period of time no hesitation is felt, in the comparatively insignificant Glacial period, of sending the surface of the land thousands of feet higher, that ice may accumulate on the now low ranges, and thousands of feet lower, that icebergs may float over the submerged lands; and no difficulty is experienced in believing that it should finish its wonderful oscillations by regaining the level it had before the Glacial period commenced. It seems a burlesque on Science that such theories should be prevalent amongst our geologists, and if they were not held by philosophers they would be ridiculed as unphilosophical. Those who advocate the former existence of these oscillations of the surface are those who urge that we should not call in the aid of any but existing agencies; yet where do they now find a shore-less and a shell-less sea? Put down a dredge anywhere in the ocean within depths of less than 2000 feet, and in the small quantity of clay, mud, sand, or gravel scraped up, it will be scarcely possible to take out a tea-cupful that shall not teem with marine organisms; yet we are taught that an immense area in Europe and America has been a sea-bottom, and every part of it a sea-beach as the land rose again, without any evidence of marine life having been left behind.

But is there really no other way of getting water up to the heights we require without resorting to this extreme hypothesis? I have in former papers urged that there is—that the ice of the Glacial period flowed principally down the ocean depressions and blocked up the drainage of the continents as far as it extended, causing immense lakes of fresh or brackish water. I was first led to believe that the ice had effected this in studying the glaciation of North America, and in 1866 I advanced the opinion that the drainage of the St. Lawrence had been blocked up by the ice moving down from the north, and that thus a great inland fresh-water sea had been formed, over which icebergs floated.* I next, in 1874, applied the theory to explain the formation of the Steppes of Siberia, after I had crossed them and found that they contained fresh-water shells, and I further suggested that the ice that descended from the mountains of Scandinavia must have dammed back all the rivers of Northern Europe.† Mr. Croll informs us‡ that before this Prof. Geikie had suggested to him that if the Straits of Dover were not then cut through, or were they blocked up by land ice, "say by the great Baltic glacier crossing over from Denmark, the consequence would be that the waters of the Rhine and Elbe would be dammed back and would inundate all the low-lying tracts of country to the south; and this might account for the extraordinary extension of the loess in the basin of the Rhine, and in Belgium and the north of France."

Very soon after my paper was published in the "Transactions of the Geological Society" I became dissatisfied with the explanation I had offered, because I found that in Devonshire there were drift pebbles and transported boulders up to heights of 1200 feet above the sea, and also that the ice of Scandinavia had actually retired before the boulder clay was spread out: as in Sweden, the latter overlies the glaciated rocks and the till left by the land ice. This is also the case on the north-east coast of England, as shown several years ago by Mr. Richard Howse. The Scandinavian drift of the coasts of Durham and Northumberland was deposited after the land ice, that had scored and grooved the rocks below, had melted back, and when the land was covered with water over which icebergs floated, carrying the erratic blocks.

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* Trans. Nova Scotian Institute of Natural Science, 1866, p. 91.
† Quart. Journ. Geol. Soc., 1874, p. 490.
‡ Climate and Time, p. 452.

During a visit I made to North America, in 1874, I found many more proofs that the drainage of the north-eastern part of that continent was blocked up by ice that flowed down the bed of the Atlantic from the direction of Greenland, and I learnt from Prof. James Hall that Cape Cod was a huge terminal moraine. Knowing, also, that on the European side of the Atlantic Mr. Robert Chambers had been led to the conclusion that Scotland had been glaciated from the north-west, that Mr. Thos. Jamieson had shown that Caithness had been overflown from the same direction, and that there was much other evidence pointing to the same conclusion,* I was led to believe that the drainage of Europe had been blocked up—not by Scandinavian ice, but by that which occupied the bed of the Atlantic and had reached to our western shores.

If we try to picture in imagination what would happen if the Glacial period were to return again, the continents keeping their present form, we shall not find it so improbable as it seems at first sight that the ice should advance down the ocean beds and be thicker there than on the land. The first effect would probably be a great increase of the ice on areas such as Scandinavia and the Alps, where it exists at present, but especially on Greenland, which lies at the northern end of the great evaporating basin of the Atlantic. For the ice would accumulate fastest where the frozen precipitation was greatest, and without great evaporation there could not be great precipitation. We need not carry on in imagination what happened in Scandinavia and the Alps, for we know what did occur, and can trace the great extension of the ice by the marks it has left on the rocks it passed over. We need only, for the present, try to follow the progress of the far greater accumulation of ice that was taking place in Greenland. When it there reached to such a height that it intercepted all the moisture of the currents of air travelling over it northwards, the precipitation would be confined to its northern slope, just as the Atlantic slopes of Central America drain the north-east trades of their moisture, so that they are dry winds on the Pacific side. But as the precipitation would not, as in Central America, run down immediately to the sea, but be retained on the southern slope of the frozen mass in the shape of snow and ice, the effect must have been that that southern slope would advance southward; so that the ice would progress not only by flowing southward as a glacier, but by the area of frozen precipitation being moved southward. Let us suppose that a ridge of ice 6000 feet high, on Greenland, would intercept all the moisture of the southern winds; then, if the precipitation every year on its southern slope was greater than what it lost by melting and evaporation, it must have advanced southward, and no depth of the ocean bed would prevent it doing so, so long as the excess of frozen precipitation continued.

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* See Quart. Journ. Geol. Soc., vol. xxxii., p. 85, where a portion of the evidence I laid before the Society has been published.

Some geologists urge as an objection to the theory of the great advance of circumpolar ice, that it cannot be pushed up a slope by a force acting from a distance. In the words of the most eminent of these, the Duke of Argyll, "the theory assumes that masses of ice lying upon the surface of the earth more than mountain deep would have a proper motion of its own, capable of overcoming the friction not only of rough level surfaces, but even of the steepest gradients, for which motion no adequate cause has been assigned, and which has never been proved to be consistent with the physical properties of the materials on which it is supposed to have acted."* If this was a fair statement of the views of glacialists a very strong case would be made out against them, for it is evident to any one having a moderate knowledge of mechanics and of the physical properties of ice that it could not be moved in such a manner; and Prof. James Forbes proved many years ago that, even when flowing down the steep slopes of the Alps, the bottom layers of the ice are so retarded by the friction of the rocks beneath them that the upper ones slip over them, producing the structure he named "frontal dip." But it is not a fair statement of their views. Mr. Thos. Jamieson, one of the greatest authorities on the glaciation of Scotland by land ice, showed, in 1865, that if snow was heaped up it would spread out at the base, and flow off "not so much on account of the inclination of the bed on which it rested as owing to the internal pressure exerted by the immense accumulation of snow,"† and he further compared its movement to that of a heap of grain which flows off when poured down on the floor of a granary, the grains flowing over each other and spreading out with a very small pitch or slope. Prof. Dana, too, has entered very fully into the question of the motion of land ice, and has shown that no slope of the land beneath is necessary so long as there is a slope of the upper surface of the ice itself down which it moves, and he has likened its motion to that of heaped-up pitch—a simile that Prof. James Forbes had also used.* There may be some geologists who believe that ice is pushed along as a solid, but certainly no such opinion has been advocated by leading glacialists, who only claim that if ice be heaped up it will flow outwards. The margin of the ice would, however, be pushed forward by the ice behind and above it in summer, when its fluidity was increased, just as in a railway-cutting we often see, in wet weather, clay slip down and push up the earth at its base.

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* Nature, vol. xiv., p. 436. Report of Address to Brit. Assoc. at Glasgow.

† Quart. Journ. Geol. Soc., vol. xxi., p. 166.

In my theory I assume that when ice in Greenland was heaped up high enough to intercept all the moisture of the winds blowing northwards, the snow would be heaped up on the southern slope, and that, if the frozen precipitation exceeded the liquefaction and evaporation, the icy mass would move southward, and the highest portion of its crest would reach to lower and lower latitudes. It would be a ridge of ice and snow gathering to itself the moisture of the atmosphere, its very growth supplying a basis for future extension, like an invading army gathering its supplies from the country it was adding to its domain. From this ridge, as from a mountain chain, the ice would flow down, chilling the atmosphere as far as it reached, and carrying with it its own wintry climate. There are tree-less districts in America where there is not sufficient rain to support forest life. Yet if a small extent of country be planted, and sustained until the trees grow up, they will cause a greater rainfall. For the rain that does fall will not be allowed to run off the land to the sea, but much of it will be retained and returned again to the atmosphere by evaporation; and as the amount of rain depends primarily upon the amount of moisture in the air, precipitation must be increased by increased evaporation. Thus a small forest may cause sufficient moisture to fall and to be retained, so as gradually to increase its limits and change a nearly tree-less country into one covered with forests. And so at the present time the ice is prevented from advancing into the temperate regions by a nice balance between the amount of frozen precipitation and of liquefaction; but if that balance were only a little shaken in favour of the accumulation of the ice, it would obtain powers of self-sustenance and growth that might cause it to spread like a living organism.

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* *See* Dana's Manual of Geology, p. 536, for an excellent account of the motion of land ice.

The height of the ridge of ice and snow would increase as it advanced southward, for it would depend upon that to which the moisture would be raised before it was precipitated, and this would be greater in low latitudes than in high.

I believe it is essential in any explanation of the facts of the Glacial period to bear in mind that from its commencement to its culmination the zone of frozen precipitation was gradually advanced southward in the northern hemisphere. On the Continent of Europe it was moved from Scandinavia and the Swiss Alps to the Pyrenees and the Maritime Alps, and when the ice was highest on the latter ranges that on the former must have shrunk back on account of the precipitation being intercepted further south, just as there are no glaciers now on the Altai Mountains because the moisture is intercepted by the Himalayas. As the ice gathered on the Maritime Alps, the Pyrenees, and probably on the Cantabian Range, the glacial ridge of the Atlantic was gradually advancing southward, and at last I suppose it coalesced with the ice of the Pyrenees or of the Cantabian Range. I thought, up to a recent period, that the Atlantic ice might have reached the coast of Europe near Brest, and so blocked up the drainage of the northern part of the continent, but I obtained proof during a visit to the valley of the Rhone this year, that it also was included within the area of the Great European Lake. This would be effected by the ice of the Atlantic glacier and that of the Pyrenees or of the Cantabian Range meeting on one side, and that of Pyrenees and of the western prolongation of the Maritime Alps on the other. Or the ice on the Cevennes may have met that of the western Maritime Alps and that of the Pyrenees; or the ice of the Atlantic glacier may have flowed across the low stretch of country north of the Pyrenees. How, precisely, this gap was blocked up I do not yet know, as I have not been able to examine it for myself, and I have had to work out the whole problem of the advance of the Atlantic ice and its interception of the European drainage, alone and unaided.

The Atlantic ice has left traces of its progress on every island and coast of the western side of Scotland and Ireland. The glaciers of the Pyrenees have been traced down unto the low plains to the north of that range,* and with regard to those of the Maritime Alps Mr.

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* C. Martins, Revue de deux Mondes, 1867.

Moggridge states that he has found abundant proofs that they descended to the level of the present Mediterranean, near Mentone.*

If, at the time the Atlantic ice and that of the southern ranges I have mentioned blocked up the drainage of Europe, the upper end of the Northern Pacific was also filled with ice, or only so far as the mountains of Kamtchatska, a great continental basin of water would be formed, rimmed in on its southern side by the ranges of mountains that extend from the extreme north-east of Asia to the Alps. The only break now existing in that rim is through the Bosphorus, and both physical geographers and naturalists have come to the conclusion, on independent grounds, that the Black Sea did not communicate with the Mediterranean until after the close of the Tertiary period, and the excavation of the channel that connects them must have been effected about the time of the Glacial period. Probably it was made when the whole drainage of northern Europe and Asia was intercepted and turned in that direction. That the waters of the Great Lake flowed that way is indicated by the Scandinavian drift having been carried far over the plains of Russia, and by the vast extent of diluvial clay spread out around the north-western shores of the Black Sea.

In this way I consider the waters were raised, over which floated icebergs from the north carrying the Scandinavian drift, and into this great lake the Danube and the Rhine, or the upper portions of them above its level, brought down fine mud from the glacier-capped Alps, which was deposited as loess. The waters everywhere were muddy, for glaciers were still triturating the mountains of Scandinavia, and thus the fine clay was spread over Europe as far as the lake extended. The very fact that the glacial waters carried fine mud in suspension to such great distances is another proof that they were fresh, for it has been abundantly proved by Dr. Sterry Hunt† and others that in salt water mud is quickly precipitated, whilst in fresh it remains a long time in suspension.

I think there is evidence that the lake reached a height of 1700 feet above the sea, and that it remained for a long time at about 1200 feet. It was once completely drained; at first gradually, but from about 500 feet above the present level of the sea suddenly and tumultuously by the breaking away of the icy barrier, and thus was produced a great deluge or debacle that swept over the lower lands and covered them with a mantle of false-bedded sands and gravel. The evidences of this debacle were insisted upon by the last generation of geologists, and they are to be found everywhere over the south of England; but of late years they have been almost ignored, Prof. Prestwich being, I think, the only one who still advocates the old theory.

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* Proc. Geol. Soc., London, 1876, p. 127.

† Proc. Boston Soc. Nat. Hist., February, 1874.

After being thus broken, the icy barrier soon closed up again, and the great lake was reformed, and this time was much more permanent, and probably existed until the communication between the Black Sea and the Mediterranean was cut through.

The areas with which this paper has had especially to deal are at an altitude of more than 500 feet, and no traces of the debacle could be expected, though I think there is evidence of the more gradual lowering of the upper part of the first lake in the separation of the loess into two divisions, as at Wurzberg. It is to the first rising of the waters that I attribute the destruction of the mammoth and the woolly rhinoceros, and probably of palæolithic man in Europe. The evidence is perhaps not so conclusive with regard to palæolithic man, but as concerns the two great quadrupeds it is clear and decisive. I can find nowhere in Europe a trace of their existence after the first rise of the waters. In the great debacle their bones were carried and spread out over the low grounds along with the lowland gravel, and doubtless often carried unto the top of low-lying patches of boulder clay, but in these cases they are broken, single, or rolled. And in the valley of the Rhone, as the great advance of the Alpine glaciers preceded by a long time the culmination of the ice of the Atlantic and Pyrenees, and the destruction of the great mammals did not take place until the approach of the latter event, there are apparent proofs of their post-glacial existence, but the evidence only shows that they lived after the culmination of the Alpine ice, not after that of the Glacial period. The evidence of the latter is not the glaciation of the rocks of the Rhone above Lyons, but the gravels and clays spread out in the same area by the great lake when there was a high ridge of ice to the southward and that of the Alps had shrunk back.

Probably palæolithic man, the mammoth, and the woolly rhinoceros survived through the culmination of the Glacial period in Asia, though the two quadrupeds soon afterwards became extinct. Mr. Boyd Dawkins and Sir John Lubbock have given cogent reasons for supposing that the Eskimos are the descendants of palæolithic man, and they still survive in north-eastern Asia. I found evidence when I crossed

Siberia in 1873 that the high lands to the north of the Altai had not borne glaciers even in the Glacial period, and they would thus afford a refuge from the great flood. But in western Europe most of the land that was not submerged was covered with ice, and there were few places of safety to flee to. What few there were, were in the east of Europe, and therefore it is, I think, that after the Glacial period passed away the animals that survived mostly spread from the south-east across Europe again, and only a few had reached England, and still fewer Ireland, before the waters of the ocean resumed their old channels, and cut off the communication with the Continent.

After the Glacial period, neolithic man, who had probably lived to the south and south-east for ages before that time, found central and northern Europe open to him. Not only the mammoth and the woolly rhinoceros had been destroyed, but, what was of much greater importance to him, the great Carnivora—the cave bear, the lion, and the tiger. The whole of northern Europe had also been covered by the fertile mud deposited from the Great Lake, and thus in every way the conditions of existence had been made more suitable for him.

The Glacial period is thus invested with a double interest; it is the first step backward in geology, the first forward in archæology, and in neither sciences can we make our footing secure until we clear away the doubts that beset us here. I claim for my theory that it shirks none of the difficulties and embraces all the facts. I have only dealt with a few of the problems in this and other papers, but I hope to find time to show in future communications that the theory affords a key to all. I can only trust that through time the faith of geologists in great upheavals and depressions of the earth's surface within a comparatively short period, in the possibility of the ocean having covered vast continental areas and retired without leaving any marine remains behind it, and in a succession of glacial periods having alternated with inter-glacial warm ones, will be shaken, and that they may look more favourably upon a theory that explains all the phenomena by one great advance southwards of the ice of a single glacial period.

VI. PHYSIOLOGY AND ITS CHEMISTRY AT HOME AND ABROAD.

By Chas. Thos. Kingzett and Henry Wilson Hake.

THE acquisition of any new and important fact in any branch of Science, in affecting more immediately that special branch whose pursuit has led to its discovery, exerts a further influence throughout all Science, which, in its gradual but universal action, may not inaptly be compared to the unlimited progress of a concentric wave. Not a few sciences owe their origin to the discovery of a single fact. Thus the discovery of Frauenhofer's lines gave rise to the great Science of Celestial Chemistry. In similar wise the Chemistry of the Carbon Compounds dates from Liebig's elaboration of the methods of organic analysis. So great was the impulse communicated by this perfection of method that from darkness it has led us to darkness again; for, in teaching us the composition of bodies, it has brought us to isomerism. When a new method of investigation shall impart a new impulse we shall again emerge into light. The influence thus originating with Liebig did not extend alone to pure chemistry; at the same time an impetus was given to Chemical Physiology, so brilliantly exampled by the work of Liebig himself. Progress has not, however, in the case of this Science been nearly as rapid as in the sister Science, and for obvious reasons. While Chemistry itself was by no means a new science, Physiology scarcely merited its name. Up to the time of which we are speaking physiologists had concerned themselves only with the study of anatomy and life functions, so far as these could be observed and explained without the aid of chemical science. Physiology, then, was at that period a science of observation and deductive reasoning, while metaphysical speculations entered largely into its narrow sphere. Liebig perceived that it was futile to appeal to the preconceived opinions of his contemporaries, and as regarded their metaphysical views he argued that such must render men powerless in time to perceive the relations of cause and effect. He devoted himself with much earnestness of purpose to expose the unscientific methods of reasoning and research then so prevalent among physiologists; he therefore addressed himself to a younger generation, and strove to impress their more plastic minds with the principles of that inductive method of reasoning so ably interpreted by J. S. Mill, and of which he was so professed an admirer. Thus he gathered around him a group of men who, by adopting and disseminating his teachings, gave rise by their labours to a new epoch in Physiology and its Chemistry. And this brings us to the subject of our Essay. Let us put ourselves at once *en rapport* with the reader as regards our immediate object in writing it. Since the time above alluded to, while opportunities for advancement have been daily increasing, the study of Chemical Physiology has, in many respects, deteriorated. There is a necessity for reform. A reference to a few researches in Physiological Chemistry, published in certain leading scientific journals, will serve to substantiate this statement. We will commence by considering the alleged discovery of a new body, to which has been given the name of "nuclein." We shall enter into a minute account of this body, because its history is typical of a hundred others of a like kind. Its first appearance was in 1871, in which year a paper by F. Miescher* appeared in "Hoppe-Seyler's Med. Chem. Untersuchungen." It was regarded as a peculiar albuminous body, rich in phosphorus, and as constituting the principal component of the nuclei in pus-cells. Lubavin* next obtained an "entirely similar body" by long-continued digestion of milk-casein with artificial gastric juice. Dr. P. Plosz* states to have obtained the same body from the blood-corpuscles of birds and snakes, and also from brain albumen. Hoppe-Seyler* obtained, further, a substance from yeast-cells which he compares to "nuclein." Ignoring details, for the moment, "nuclein" is said to be obtained from these various matters after they have been freed from substances soluble in water, dilute acids, alcohol, ether, &c., by a process of artificial digestion with dilute hydrochloric acid and pepsin. The body in question is soluble in alkaline solutions, and has, as admitted even by those who have worked with it, the further general characters of a form of albumen. Thus it gives a xantho-proteic reaction with nitric acid, and its soda solution gives no reactions which can be said to be characteristic of a new substance. From this it will be seen, the only evidence that the body is at all new to science consists in its contained phosphorus. More recently† Dr. Miescher, of Basel, asserts once more to have obtained this "nuclein" accompanying "protamin" (of which more anon) in the spermatozoa of some vertebrates. Finally, its presence in human brain is re-asserted by Rudolf von Jaksch,* of St. Petersburg, a student in Hoppe-Seyler's laboratory. We here reproduce, in tabular form, the percentage composition of these various preparations, as given by the several authors :—

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* Maly, Jahresbericht der Thier-Chemie für 1874. Wiesbaden, 1875.
† *Ibid*, p. 337.

	From Yeast-Cells, by Hoppe-Seyler.	From Milk-Casein, by Lubavin.	From Pus-Cells, by Hoppe-Seyler.	Pure Nuclein from Spermatozoa, by F. Meischer.	From Brain, by Jaksch.		Analysis given in Hofmann's "Lehrbuch der Zoo-chemie."
C =	43·00	48·50	49·58	36·11	50·60	50·50	49·60
H =	6·06	7·15	7·10	5·15	7·40	7·80	7·00
N =	15·31	13·30	15·02	13·09	13·12	13·15	14·00
P =	2·58	4·60	2·28	9·59	2·08	1·71	2·50
O =	—	26·45	—	36·06	—	—	25·10
S =	—	—	—	—	—	—	1·80

Some of these authors write of a modification of "nuclein," insoluble in alkalies, in addition to the above. The following table shows the percentage composition of some forms of albumen :—

	Albumen.	Mucin.	Gelatin.	Fibrin.	Syntonin.	Lardacein.
C	53·5	52·4	50·16	52·7	54·1	53·6
H	7·0	7·0	6·60	6·9	7·2	7·0
N	15·5	12·8	18·30	15·4	16·1	15·0
S	1·6	Nil.	0·14	1·2	1·1	1·3
O	22·0	27·8	24·80	23·5	21·5	23·1

If these figures be studied, side by side, with those attributed to "nuclein," it will be seen that the percentages of hydrogen, nitrogen, and sulphur (where it occurs) in "nuclein" are precisely those which belong to various, and not unlikely impure, forms of albumen. Further, the phosphorus ranges in "nuclein" between 1·71 and 9·59 per cent, while the carbon ranges between 36·11 and 50·60 per cent. As we shall proceed to further demonstrate that this "nuclein" is a form of albumen, we may at once explain the absence of sulphur in most of the preparations by the fact that the authors heated them with caustic soda or potash, which would remove the sulphur from many forms of albumen : in fact this constitutes the method of preparation of alkali albuminate. As for the phosphorus, we contend that its presence proves but one thing, viz., the impurity of each of the above preparations containing it. How it exists there is also readily explained. Lecithin is a known constituent of each of the matters from which "nuclein" has been derived; and confining our attention to the human brain, this may serve to illustrate all cases. Jaksch directs the brain to be skinned, washed with water, passed through a sausage-machine, then to be extracted successively with cold alcohol and cold ether; after which it is to be pressed, rubbed, and extracted with absolute alcohol, and filtered. The residue is then to be digested with extract of pig's stomach and dilute hydrochloric acid during forty-eight hours, at 40° C.; the residue is extracted with soda, and the amber-coloured solution yields "nuclein" on precipitation with hydrochloric acid, accompanied, as the author states, by the evolution of a most peculiar odour, which he suggests may be sulphuretted hydrogen! Without claiming absolute purity for his preparation, he entertains no doubt "that it is a nuclein kind of body in a Miescher" (why not "Pickwickian?") "sense."

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* PFLÜGER'S Archiv., vol. xiii., p. 469.

Now, in considering a little more closely the properties of this substance, we find that before it will give the phosphate test with molybdic acid it has to be boiled with stronger acid. Our acquaintance with brain-matter guarantees us personally in saying that Jacksch's proceedings would leave behind in the brain residue very considerable traces of myeline, and perhaps lecithin. Without pausing to explain why this is, we know that any phosphorus left behind would, on boiling with an acid solution, be yielded as glycero-phosphoric acid, and this with molybdic acid would give the reaction which so astonished Jaksch. We tarry only to characterise one other statement made by Jaksch, which may be given in his own words:—"It will, however, be easy to estimate quantitatively, as nearly as possible, the amount of nuclein in the brain, from the standpoint that in the brain no other phosphorised principles occur except lecithin and nuclein. One requires only to remove the lecithin from the brain, and to subject the residue to a calcium and phosphorus determination."

The fact that the above was printed after the publication of Thudichum's researches in brain chemistry* intensifies rather than palliates the ignorance it displays. We are compelled to refer once more to F. Meischer. In a later paper† this author states the formula of nuclein, from the analysis above quoted, to be $C_{29}H_{49}N_9P_3O_{22}$, and opines that the body is a tetrabasic acid. Indeed he states that he has calculated these figures and formula from a barium salt, for which he only gives the percentage of barium. He further says that it may combine with sodium and "protamin" in various different ways, giving rise to many definite and new striking compounds. Accompanying the "nuclein" from Spermatozoa was a base (the above protamin) whose hydrochlorate was difficultly soluble in alcohol. From the analysis of some platinum salts he has calculated a formula for the free body (protamin) of $C_9H_{20}N_5O_2$, but in the specific analyses from which he has made these calculations he has not given the percentages of platinum and chlorine found. Taking, however, one of his analyses as follows—

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* Med. Off. Rep. Privy Conncil and Local Government Board, New Series, No. III., p. 113.

† Jahresbericht der Thier-Chemie (MALY) für 1874. Wiesbaden, 1875.

C = 23·160
H = 4·355
N = 15·000
O = 6·252
Pt = 24·643
Cl = 26·590,

and calculating out these figures in the ordinary way (Pt = 1), the following empirical formula results :—

$$C_{15}H_{33}N_8O_3,2HCl,PtCl_4.$$

In this computation we have calculated his chlorine on the assumption that it is associated with the platinum in the relation 6 : 1 ; for in no analysis does he give determinations of all elements present in the molecule of his preparation. That his formula for protamin is therefore incorrect, on the surface of things, is proved by a mathematical consideration of the data furnished by the author himself. Piccard, in the same journal, shows that what Meischer really had in his hands was a mixture of sarkine and guanine, though this critic admits that beyond these substances there certainly was a new base. We would suggest for that "new" base the old name, which it has always borne, of lecithin or of neurine. We shall dismiss this subject with the remark that, as regards these "nuclein" and "protamin" precipitates, they may not unfitly be classified, together with certain human beings, under one comprehensive heading—the Great Unwashed.

More seriously we feel that, as a result to Science, whatever might have been valuable in these researches is, for the time being, absolutely valueless.

Another example of similar work is presented in a paper by Arcadius Rajewski* (of St. Petersburg), "On the Occurrence of Alcohol in the Organism." The experiments were conducted in Hoppe-Seyler's laboratory. Alcohol was injected into the stomachs of rabbits, and after some hours the animals were killed, their brains removed, and these distilled with water, in order to ascertain whether a final rectified distillate would give Lieben's iodoform reaction: it did so. This result induced the author to repeat the experiment upon the brain of a rabbit to which no alcohol had been administered: a like result was obtained. Other experiments were now instituted with the muscle and livers of alcoholised and "sober" rabbits, with no difference in the obtained results. Again, large quantities of ox-flesh, horse-flesh, and the flesh and livers of dogs and horses, were similarly treated; in each case a fluid distillate being obtained which gave the iodoform reaction, &c., but "which would do anything but burn."

The conclusions drawn by the author from these experiments are as follows:—"That for the estimation of alcohol in the system Lieben's reaction is of no use, since in the animal organism there are portions existing which on distillation give alcohol; or the organs of animals contain an ever-preformed quantity of alcohol."

Now, it is well known that Lieben had himself found the reaction which bears his name to be of no use for detecting alcohol in the urine.

In further reference to this subject we would add that Dr. Percy, in 1839, published a research on the presence of alcohol in the ventricles of the brains of animals to which it had been administered, and concluded that a kind of affinity existed between the alcohol and the cerebral matter. Every other result of Rajewski's had been previously arrived at and published by Dupré and others, and more correctly interpreted. These observers showed conclusively that a substance exists in the animal body, and could be obtained therefrom, which gives many of the reactions of alcohol, but which is not alcohol. It is certainly desirable that experiments should be conducted with a view of ascertaining the extent to which alcohol may locate itself in the brain; but this is not to be done in the way Rajewski has attempted it. It may here be stated that one of us is at the present time occupied with this matter.†

Another typical research will suffice to illustrate a sensational aspect of the inferior kind of work presented by the scientific literature of our day. The author, F. Salmi, professes, in a discursive paper* of great length, to have succeeded in isolating "an alkaloid occurring in the brain, the liver, and the wild poppy." He appears to have obtained a substance from each of these sources which may have been anything in each particular case, for it presents no reaction stamping it as a new individual. Nor does Signor Salmi give a single analysis of any kind.

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* PFLÜGER'S Archiven, xi., 122.

† KINGZETT, "Alcohol on the Brain," Chemical News, xxxiv., p. 158.

In leaving for a time the consideration of misguided research, we turn for awhile to consider the educational literature of our subject, and this will be best done by reviewing in brief certain typical text-books of modern physiological chemistry.

In 1873, among other text-books published in this country, was a "Handbook for the Physiological Laboratory,"† by Dr. Klein, Prof. Burdon Sanderson, Dr. Michael Foster, and Dr. Lauder Brunton. The Report of the Medical Officer of the Privy Council, containing the Brain Chemistry, had certainly not been published when the Handbook appeared: still the literature of Brain Chemistry was, at that time even, sufficient to form the basis of an ordinary octavo volume; but in the 572 pages of the Handbook we find little more than one page is devoted to the chemistry of the brain. In that part of the Handbook devoted to the Chemistry of the Tissues, by Dr. L. Brunton, the student is directed to estimate the amount of water in brain-matter by drying it over sulphuric acid, and, further, to grind the brain to paste in a mortar. The former is, in our opinion, as impossible as the latter is unfeasible. Following the methods here laid down the student would not succeed in isolating a solitary brain principle in a pure state, while with regard to that one principle, viz., cerebrin, about which the instructions are most minute, operations are detailed for its purification which must inevitably result in its destruction. We are told that to obtain pure cerebrin it should be, among other things, boiled with baryta water. Now this proceeding splits up cerebrin into sugar, a new base, and certain other products. Moreover, cerebrin is not hygroscopic, as stated by Dr. L. Brunton. We take this opportunity of paying a tribute of respect to the valuable researches of W. Müller relating to the discovery of cerebrin.[*] As a matter of history it is interesting to recal how preference was given by his contemporaries, among them his own friend Von Gorup-Besanez, to the work of Liebreich,[†] and how since that time Liebreich's protogon has been proved to consist of a mixture of cerebrin and myelin,[‡] while Müller's cerebrin remains a definite individual. Space alone prevents our entering here into observations regarding the extent to which the action of baryta upon cerebrin may have misled Müller, for with that action he was not acquainted. In those pages of the Handbook devoted to Saliva we meet with the following passage:—"When poured from one vessel to another," says Dr. L. Brunton, "it (saliva) is seen to be more or less viscid, in consequence of which it is generally filled with air-bubbles. If none are present they are readily produced by blowing into the liquid by a narrow glass tube, when it is seen they take a long time to subside." And somewhat later, referring to methods of stimulating the secretion of saliva, he recommends one to "collect a considerable quantity, by filling the mouth with ether;" and again, "roll a pebble or a glass stopper in the mouth, and attempt to chew it." Another matter to which we take exception in this part of the book is that, while Thudichum has worked more than any other chemist living on the biliary colouring-matters, his name does not occur in connection with this subject; indeed the only mention of his name throughout the book consists in a bare reference to his treatise on the "Pathology of the Urine." Dr. Brunton gives Städeler's formulæ for bilirubin ($C_{16}H_{18}N_2O_3$) and biliverdin ($C_{16}H_{20}N_2O_5$). These formulæ had been withdrawn by Städeler at the time when Dr. Brunton wrote, and he had substituted other formulæ which were theoretically constructed on a perversion of Thudichum's own results. The true formulæ for these substances are—for bilirubin $C_9H_9NO_2$, for biliverdin[||] $C_8H_9NO_2$. Kryptophanic acid, which was proved by Thudichum to constitute the normal free acid of human urine ($C_5H_9NO_5$), and whose compounds have been thoroughly worked out, is not even mentioned in the Handbook. On the other hand, indican ($C_{16}H_{12}N_2O_2$) is said to be ever present in urine. We have personally examined many different specimens of human urine, and find the above statement to be without foundation. Schunck, on whose researches this statement was originally based, obtained from indigoferæ a principle which on treatment with acids split up into indigo-blue on the one hand and sugar on the other. It was therefore supposed to be a glucoside which furnished these matters. Subsequently, by suitable treatment, he claimed to have demonstrated the presence of a similar substance in normal urine. To this substance was given the name of indican; but its presence in urine at any time is extremely doubtful, for in the first place no such substance has ever been isolated from urine, and, in the second place, when that blue colour is obtained which is supposed to be characteristic of indigo-blue and referable to indican, side by side with it *sugar is not produced*. When a solution is obtained which exercises a reducing action on Fehling's copper solution, there is every reason to believe the case abnormal in some respect. When urine is treated with its own volume of hydrochloric acid, a blackish precipitate falls on standing, and this, after extraction with dilute acid and then with absolute alcohol, is said to leave indigo-blue upon the filter. We have rarely met with cases when any blue is obtained, and when it has been obtained it has amounted to little more than a stain. But it will be evident that in any case it is not justifiable to speak of indican as a never-failing constituent of urine. Further, the name should no longer be retained, for if indican be a glucoside, and if the blue colour from urine be unattended by sugar, then at least the substance in the urine cannot be indican.

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* Gazzetta Chim. Ital., v., 398.

† Handbook for the Physiological Laboratory, by E. KLEIN, M.D.; J. BURDON SANDERSON, M.D., F.R.S.; MICHAEL FOSTER, M.A., M.D., F.R.S.; and T. LAUDER BRUNTON, M.D, D.Sc. Edited by J. BURDON SANDERSON. London: Churchill. 1873.

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[*] Ann. Chem. Pharm., 105, 361 (1858).

[†] *Ibid*, 134, 29.

[‡] Med. Off. Rep. Privy Council, New Series, No. III., 1874.

[||] *See* "State of Animal Chemistry in Austria," by J. L. W. THUDICHUM, M.D. Chemical News, April 13th and 21st, 1876; and Manual of Chemical Physiology.

It is pleasant to be able to speak in eulogistic terms of Dr. L. Brunton's clear and succinct account of the bodies known in chemistry as albuminous. Such a task is as difficult as it is unthankful, for arranged under this heading there are almost endless forms of matter which, having many characters in common, have such complicated molecular constitution that hitherto it has defied the attempts of all who have worked upon the subject, to construct a formula for any one of them which is entitled to absolute acceptance. But it will ultimately be found possible to assign such a general constitutional formula to these substances that, while it may characterise no one individual body, it shall yet include and represent all. In this direction the researches of Schützenberger on the albumens, and Sullivan in other directions, encourage us in the hope that the day is not far distant when this may be accomplished.

We are strengthened in this conviction by our increased knowledge of the chemistry of the brain. Here occur groups of phosphorised principles which, for the time being at any rate, we may view in a parallel light. For many years we knew nothing of the form in which phosphorus exists in the brain, and, having passed through that stage of its history when its presence was explained by definition, Gobley discovered egg-lecithine, and proved that, by means of chemolytic agents, it was split up into glycerophosphoric acid, a base, and fatty acids. Next, Thudichum discovered other principles in the brain containing phosphorus, namely, kephalin and the forms of myelin; and on continued investigation it has transpired* that all these principles are constructed on the same type, and may be said to be glycerin either in its simple or condensed forms, in which hydroxyls are replaced by fatty acid radicles on the one hand and phosphoryl on the other; the phosphoryl in its turn having hydroxyl replaced by a nitrogenised basis radicle.

It is only in the possession of such knowledge that we are able to appreciate to the full the enormous difficulty experienced by investigators concerning like highly complicated molecules, for the difficulty in the instance we have given is increased through the previously unknown nature of some of the fatty acid radicles. Moreover, the fact that we are at length able to classify such groups of bodies is by no means the least proof of their chemical individuality, for it places in the hands of the worker a law which must be obeyed, if the body to which it is applied belongs to that class.

This brings us again to the albuminous bodies of which we were treating, and to a study of a line of research which —so far as it has yet extended—has led but to insignificant results. We refer to the action of pepsin upon albuminous substances. Of pepsin itself we may be said to know nothing, except that from time to time statements crop up in current literature purporting to the alleged discovery and isolation of the actual ferments contained in it. But no analyses are forthcoming, and these excitements are short-lived, and leave us once more in our old position. Yet it must be acknowledged that the researches of Erlenmeyer (in part published) and of Alex. Schmidt promise to throw much light upon these subjects. It is not surprising, then, that our knowledge of the peptones is no greater. It seems to be the object of certain men to test the action of pepsin upon all sorts of albuminous bodies in acid solutions, in order that when they have obtained their products they may pass them through a certain number of tests. These tests may be said to consist of the precipitability by neutralisation, boiling, or by stronger acids, and by such reagents as potassic ferrocyanide and cupric sulphate. But while they recognise that these peptone solutions give precipitates with bodies like tannic acid, argentic nitrate, mercuric chloride, platinic chloride, and plumbic acetate, it seems never to occur to them that it would be more profitable to follow up any one of these operations, and by so doing obtain, at once and for ever, a definite principle by definite means. As it is, however, our literature and our text-books abound with descriptive matter concerning what may appropriately be termed "test-tube chemistry." The test-tube as an agent in pioneering experiments, as a preliminary to the application of elaborated methods of research, based, indeed, in the first instance, upon such reactions, is invaluable; but employed as a final test of character and constitution, these reactions are absolutely without meaning. A precipitate or a colour may result from the mixture of two solutions, but no amount of repetition of such reactions can teach us more than the experience of the fact. To the precipitate we must apply methods of purification, and analysis, in order to ascertain its composition, and for the colour we must use the spectroscope, or other means at our command which shall in like manner identify for us its nature. But when we have effected all this we have but begun, for it yet remains to learn the inner constitution of the body whose empirical formula we have established; even then we have yet to determine in what way it was in the first place produced from the mixture. Then, and not till then, are the resources of the chemist exhausted and the problem solved. Now, by some of the reactions above indicated, it will appear reasonable that phospho-molybdic acid would precipitate many peptone solutions, in which case it is not unlikely that by the subjection of the products to decomposition with baryta water, and further treatment of the free substances, some very definite results would be obtained. Meanwhile a general name represents all we know of substances which will one day prove to have a constitution, complicated certainly, but as evident and as well defined as those of the best-known bodies.

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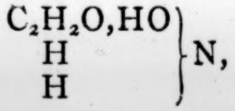
* THUDICHUM and KINGZETT, "On Glycero-phosphoric Acid and its Salts, as obtained from the Phosphorised Constituents of the Brain." Journ. Chem. Soc., Ser. 2, No. 163.

We now pass on to consider another text-book of physiological chemistry,* by C. H. Ralfe. In this book, leucin (termed here a glycolamid)—



$$\left.\begin{matrix} C_2H_2O,HO \\ H \\ H \end{matrix}\right\} N,$$

is alluded to as a neutral body which does not form salts. Now it not only forms salts by combination with acids and bases, but it is moreover capable of expelling hydrochloric acid from cupric chloride : moreover, the formula the author quotes is itself explanatory of these facts. More startling, however, is the statement that the vegetable organism is "synthetical," while the animal "analytically reduces." Were this wholly true we should be at a loss to understand how it is that hematocrystalline exists in the blood, or the principles to which we have above alluded go to build up brain-matter. In speaking of a possible mixture of glucose and chlorides, the author directs the removal of the latter by argentic nitrate, and then says—"On concentration, the glucose will crystallise out." This account would have been more correct had he for the word "glucose" substituted "nitrates," and still more correct if—instead of using nitrate of silver—he had directed the use of oxide of silver. Why, too, he should class cholesterin ($C_{26}H_{44}O$, a monatomic alcohol) as a non-saponifiable fatty principle, or speak of the fixed oils as highly inflammable (meaning, of course, their products of decomposition by heat), we are at a loss to imagine. Again, he is in error when he says that most of the albumens contain sulphur and phosphorus. As for the method he gives for separation of oleic, palmitic, and stearic acids from a mixture of the same, we cannot do better than state it in a few words. He directs to saponify with potash in the presence of alcohol, and to decompose the mixture with sulphuric acid; the removal of the potassic sulphate thus formed, by filtration; and the concentration of the alcoholic filtrate. The palmitic and stearic acids are thus stated to crystallise out, while from the mother-liquor is obtained the oleic acid by distillation, extraction of the residue by ether, and evaporation of the extract. This, it will be seen, amounts to no separation at all, for the palmitic and stearic acids are still in mixture and unpurified, while the oleic acid obtained as described will contain also some palmitic and stearic acids.

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* Outlines of Physiological Chemistry, by C. H. RALFE, M.A., M.B. London: H. K. Lewis. 1873.

We are also told that uric acid never occurs free in normal urine, while damaluric and damolic acids are given as normal constituents; whereas uric acid is a normal constituent, and damaluric and damolic acids never occur in human urine, and only on one recorded occasion have these latter been obtained from the urine of a cow.

On the subject of the brain Mr. Ralfe reproduces that which has long since been disproved, viz., Fremy's conclusions* regarding the presence of so-called oleo-phosphoric acid, a body which received in Fremy's hands not a single complete analysis, and which was undoubtedly a mixture of other substances with lecithine. This is the more remarkable, for Fremy, in claiming the existence of this body, overlooked or ignored a far more important discovery by Couerbe, viz., his cephalote, which, however, Couerbe did not obtain pure; had he done so he would have obtained the kephalin of to-day. Unfortunately an accident induced Couerbe to assert the presence of sulphur in his substance, and on this account there was denied to him that credit to which he was so fairly entitled.

It is a pity that, in our acknowledged state of ignorance as regards the chemistry of the body, such analyses as the following should be given, of an organ regarding which, in the past, our information has been so imperfect:—

Cerebral Matter.

Water	80
Fats	5
Albumen	7
Extractives and Salts . .	8

But this analysis, given by C. H. Ralfe in his book, is no worse than many similar ones to be found elsewhere. Among these extractives and salts leucin and uric acid are erroneously included. The author falls into the same mistakes regarding bilirubin and biliverdin as Dr. L. Brunton, and alarms us by saying that the breath in certain diseases contains chloride of sodium, uric acid, and ammonium urates. In this book, also, as well as in the "Handbook for the Physiological Laboratory," the spectroscopical characters of hematin, so beautifully worked out by Stokes and others, are incorrectly described.

One more point, and we have done with Mr. Ralfe. He writes—" The inorganic substances occurring in the animal tissues and fluids are not chemically combined with the organic principles." The only exception he allows, in doubt, is alkali albuminate; we had always thought bones, above all things, established the fact that some at least of the inorganic substances are in rather strong combination with the organic principles, while, as a matter of fact, so strong is the combination between kephalin ($C_{42}H_{79}NPO_{13}$) and certain salts with which it is combined in brain-matter, by virtue of its alkaloidal constitution, that it can only be obtained in the pure state after the destruction of such combinations by hydrochloric acid, which has the power of uniting with it to form a hydrochlorate.

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* Ann. Chem., ii., 463; also Journ. de Pharm., xxvii., 453.

We now come to a treatise by Prof. Karl B. Hofmann, of Graz,* and we find that in the 144 pages forming the first published half of this book there is more space devoted to the consideration of "nuclein" than to the whole brain chemistry. Moreover, although Thudichum's researches on this subject have been published since March, 1875, he dismisses them with the following words:—"Thudichum's newly-published list of numerous compounds can here only find mention." In keeping with such imperfection, we are not surprised to find the same errors that we have already pointed out as present in previously mentioned works. To be just, we will point out that whenever an opportunity is presented of entering into an enunciation of the structural chemistry of commonly well-known substances, the author never fails to do so; as an instance of which eight pages are devoted to the structural formula of glycerin, a matter which is as far removed from any *advance* in physiological chemistry as any matter well can be.

That our readers may not deem us unreasonable in demanding for Thudichum's researches in brain chemistry that respect which they merit, we would point out that, while our position has personally associated us with this work, we are obviously precluded from entering in any detail into the subject. We would nevertheless explain that it has been conducted, during a period of five years, on such a scale and in such a manner as can only be secured by the outlay of money in the power of Government alone to provide. As a consequence, it stands out pre-eminently as one of the greatest researches in physiological chemistry of modern time, and has given such an impetus to this science as has not been felt since the days when Wöhler constructed urea, and when Liebig gave to the science a new meaning and a new method. That this opinion will prevail ere long there can be no doubt; meanwhile it is the privilege of those who are more intimately acquainted with the work to assert their conscientious conviction of its value. It is indeed painful, after such reflections, to turn to a "Jahresbericht" of animal chemistry* for the year 1875, to find that the only notice afforded in these pages of Thudichum's work is taken from the "Centralblatt," which in its turn derived its knowledge from the "Chemical News." In the latter was published an abstract of a summary given orally at an ordinary meeting of the Chemical Society of London. It further appears that a communication had been made to Dr. Maly by Dr. Dreschfeld, of Manchester, whose name figures on the cover of this "Jahresbericht." Dr. Maly concludes his notice with these unworthy words:—"The critical remarks appended by Dreschfeld to his report, viz., that throughout impure smeary masses formed the bases for Thudichum's new substances and new names, cause the omission of so extensive an account to be a matter of little regret.—*M*."

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* Lehrbuch der Zoochemie, von KARL B. HOFMANN, Prof. der Phys. Chem. au der Universität Graz (Wien, 1876).

Leaving the immediate criticism of special research and current literature, we would dwell for a moment upon the accepted doctrine that the best work and the greatest number of results issue from German laboratories. Without being desirous of entering into a controversy on this matter, we wish to indicate that there are workers in England who appreciate to the full all that Liebig appreciated of the methods of science, and who are, moreover, capable of carrying out those methods. There is an universal lack of scientific work, and especially of research in physiological chemistry; but in any comparison between Germany and England, we are by no means disposed to concede the laurel to Germany. To those who are intimately acquainted with German scientific literature the fact is patent that the enormous mass of work published represents also Russian, Polish, Austrian, Hungarian, and Belgian, nay Italian, workers. It is also well known that as regards the large percentage of Russian workers, these are enabled, at the expense of their Government, to study science in Germany, and for the last twenty years all their work in physiology, chemistry, anatomy, and microscopy has been published in German periodicals in their own names. Confirmatory of our remarks we here give the following statistics:*—There were at Berlin at the beginning of the present year 1884 German students, including 162 owing no allegiance to the empire. The favourite university of the Germans is Leipzig, which has 2575 Imperial German students, of whom 939 are Saxons and 1143 Prussians. Third in rank, if judged only by the number of students, is Munich, with 1087 Imperial German students, of whom 912 are Bavarians and 113 Prussians. The distribution of students among the four principal faculties is as follows:—

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* Jahresbericht über die Fortschritter der Thier-Chemie, von Dr. RICHARD MALY, Professor in Graz. Wiesbaden, 1876.

	Berlin.	Leipzig.	Munich.
Theology	162	337	84
Jurisprudence . .	807	1130	257
Medicine	263	369	307
Philosophy . . .	911	1089	555

The difference between the sums of these columns and the above figures give us the number of foreigners not Germans studying in each university as follows:—Berlin, 259; Leipzig, 350; Munich, 116. Berlin has therefore a total of 2143 students; Leipzig, 2925; and Munich, 1203.

Now, if any comparison is to be instituted between German and English work, all these accessories on the former side must be deducted, or to English must be added American, Australian, and Indian work.

The inferior character of some of the German work seems to be referable, in part, to the system of their university teaching, for it is a fact that the professors of medical chemistry are often associated with applied chemistry; that is to say, their work lies between general chemistry on the one hand, and medical chemistry on the other. When it is therefore considered that the medical men are far behind Englishmen in their acquaintance with Materia Medica, and that these same men place in the hands of students work of a severe character in science, it is not surprising that the results should in many cases be so deplorable.

Let it be understood that this Essay is in reference to Physiological Chemistry, and that, in writing of certain untrustworthy work emanating from Germany, we have no wish to cast an imputation upon the mass of good work in pure chemical science of which the nation is so justly proud.

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* Literattische Wochenbericht, of Leipzig, of February 24, 1876.

We have advanced, in the foregoing pages, our conviction that in Physiology and its Chemistry there is a necessity for reform, and have substantiated our view by facts. We have shown that, here and there, men have lost sight of those ruling principles underlying all science, and have plunged blindly into the mazes of uncerebrated research, the conflicting results of which have reduced them to a compromise between their intellect and their honour. In pursuing research we must ever bear in mind the ultimate object of physiological chemistry, and the means by which this object may be attained.

Liebig assumed the existence of a vital force, powerful to develope from the seed the plant, and from the egg the bird; but he yet acknowledged, above all men, that the actual processes of life, with all their complications of function, were based upon the same laws which exist between matter and force in the chemist's laboratory. We cannot therefore but regard those who hold the opinion that there is something in life which renders negative all our conclusions drawn from laboratory experiments, and nullifies all our hypotheses of functions, as men who fail to grasp thoroughly the conditions upon which life depends.

The object of physiological chemistry, then, is the reduction to general laws of those phenomena which, in their multifarious co-relations, constitute the functions of life, in health and disease. To prosecute this study with hope of success it is essential that we should first become acquainted with the composition and constitution of those substances which are elaborated in the various tissues, organs, and fluids of the living body, and of which these are themselves constituted. It is only by means of such knowledge that we can be enabled to trace out the intricate concatenations of the various parts of the animal body, and of those metamorphoses which are constantly in process in the living laboratory.

From this it will be seen that the method in the study of Pathology is necessarily of the same order as that employed in Physiology, and the results of these parallel investigations appear as the expressions of health and disease.

In drawing towards the close of our Essay we admit that in the Science of Physiological Chemistry there are pending matters for research which might fairly be undertaken by men of ordinary attainments, at very little expense; but that there are questions to be solved which are further removed from the abilities of the untrained mind, and which demand for their elucidation means at the command of but few private individuals, is, we may say, notorious and acknowledged. We have endeavoured, *en passant*, to indicate some of these problems, and did space allow it would be matter of interest to allude to many others. A few may conveniently be here given. In the first place, of the matters entering into the composition of so-called extractives we know extremely little: in fact, Liebig—who has worked at this qnestion more than any other man—was only successful in ascertaining the nature of less than 17 per cent of the total matter. Fortunately for science Liebig was more conscientious than many workers in this direction; for it is a weakness not only on the part of the physiological chemist, but on that of the ordinary chemist, in their investigations, to seize upon one particular product, preferably choosing the most crystalline, and to examine it to the neglect of the 99 per cent. This would not be of so much import if at the same time it were accompanied by a statement of the fact, but as it is, their publications are not rarely so worded as to convey to the mind of the reader, imperceptibly, the idea that nothing remains to be done. Again, with remarkably few exceptions, nothing is known regarding those relations which undoubtedly exist between the chemical constituents of the various organs, tissues, and juices of the body. We will refer for the moment, in illustration, to the identity* of the phosphorised principles accompanying blood corpuscles with one of the forms of myelin as existing in the brain. This discovery, however, is but the key-note to other discoveries which must and will be effected before we can even speculate on the functions of that matter itself, either in relation to the intellectual faculty or in regard to the blood function. That there are similar relations existing between other brain components and the bodies found in the liver and the urine, we have also no doubt, but the nature of these relations, or the connection of excretory products with the albuminous constituents of the body, is almost entirely hidden. But with the increase of our acquaintance with substances already known to Science, and with the discovery of greater numbers yet to be revealed, will come the explanation. The pursuit of this science, like that of all pure science, is, it is well known, as unremunerative as it is laborious to those who undertake its investigations.

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* "On Hemine, Hematine, and a Phosphorised Substance contained in Blood Corpuscles." By J. L. W. THUDICHUM and C. T. KINGZETT. Journ. Chem. Soc., New Series, No. 165, September, 1876.

We conclude our Essay with the hope that our country—ever sensible of matters affecting the welfare of individuals and communities—may not neglect to nurture its institutions, nor forget those workers who are qualified to effect what yet remains to be done, as testified by that which they have already accomplished.

NOTICES OF BOOKS.

Aerial Navigation. By the late CHARLES BLACKFORD MANSFIELD, M.A. Edited by his Brother, R. B. MANSFIELD, M.A. London: Macmillan and Co. 1877.

THE late Mr. Mansfield was best known as an accomplished chemist, but he wrote upon various subjects, and possessed an uncommon breadth of culture. His "Travels in Paraguay" is a very suggestive book, quite beyond the usual run of narratives of travel. He was an accurate scientific worker, possessed a keen insight into the mysteries of Nature, and was moreover gifted with a fine imaginative faculty. He had something of the genius about him, and from whatever point of view we observe his faculties we see at a glance that he was no mediocrity. Some insight into the breadth of his reading may be obtained at the very outset. After the dedication of his "Aërial Navigation" "To the Industrious of All Nations," he places before his Preface a few quotations from various writers, which seem to strike the key-note of his theme: he quotes from Tennyson's "Locksley Hall," Jacob Behmen's "De Signatura Rerum," Friar Bacon's "De Secretis Operibus Artis et Naturæ," Chancellor Bacon's "Sylva Sylvarum," and Hans Christian Andersen's "Ugly Duckling." The work itself is unfortunately unfinished, and it is much to be regretted that no less than twenty-five years have elapsed since it was written; yet it is said that no book has appeared during that period which has taken the place in the literature of the subject which this volume was meant to occupy. It seems to have been thrown off from the author's brain, as a mere mental exploit, to relieve himself of a burden. "My object in writing it," he says, "will be simply to deliver my brain of a burden which came upon it uninvited."

The work is divided into three parts, of which the first states the problem to be solved, the second gives hints for its solution, while the third takes the form of some very useful appendices.

It is clear that there are two methods by which a body may be sustained in the air: first, by mechanical force applied to the air, as in the case of the flight of birds; secondly, by the attachment of a buoyant body, as in the case of a balloon. The author does not consider the sustention and propulsion of a man in the air, by his own muscular effort, an impossibility; and he further believes that a good grip of the air could not be obtained with a propelling surface and mechanism weighing less than 10 lbs., which we should have considered an extreme minimum.

A man in walking a mile has done work, as far as his legs are concerned, equal to that which would be required to raise him vertically through about 146 yards; and if we reckon that a sound man can walk 30 miles a day, he would expend sufficient force to raise himself and an apparatus of wings weighing 10 lbs. through a vertical height of 2⅓ miles. If we suppose that his wings have each an area of 7 square feet, they must move through 18 feet in a second in order to neutralise the weight of the system, and any power above this could be used for raising and propelling the body. To remain poised in the air the wings must be kept vibrating with such a velocity that they move through 25½ feet per second. We may suppose a man, without overtaxing his strength, to be able to make ten complete beats of his wings in a minute. The wings would have to be very gigantic: according to a calculation given on page 30 the distance to the centre of the wing must be 153 feet. The first essential condition of aërial navigation is stated to be—"That the propelling power must be so adjusted that it may be kept constantly horizontal, and that the force must be so applied that it shall have no tendency to make either gas-vessel or man-vessel deviate from a strictly horizontal position, and this whatever may be the speed of flight."—(P. 51.) The second condition is—"That the gas vessel must be kept perfectly stiff, so that it shall neither turn up nor be bent down at the ends, but shall keep its form under all varying conditions of load from below and of gas-charge within."—(P. 55.)

Many proposals have been made as to the shape of the gas-vessel. Sir George Cayley, in 1816, proposed that it should have the shape of a woodcock's body; others have proposed prolate spheroids and spheres; others a fish-like form. Our author concludes his chapter on the question of shape by the assertion—"The vessels of air-craft must be of an elongated form, to enable them, by cleaving the air and eluding its resistance, to receive the highest velocity attainable by the exertion of a given amount of power." The material must be impermeable to gases, and at the same time strong and light, and it must be charged with the lightest possible gas. Hence a cheap way of preparing large quantities of hydrogen must be found. The air-craft must retain its buoyancy unaltered: it must be able to retain both gas-vessel and man-vessel in a horizontal position, and to regain its level if disturbed. Then comes the great question of *power*. Numberless suggestions have been made on this score: steam, electro-magnetism, human muscular power, the traction of flying birds, &c. Then there must be appliances for transmitting to the outside atmosphere the force generated within the balloon, in such a way that there will be sufficient leverage to propel the craft forward. An interesting summary of the requisite conditions is given in the last chapter of the first part (pp. 174 to 182).

In the second part are various hints of great ingenuity and value for obtaining each of the requisite conditions. Unfortunately the work ends abruptly in the middle of the chapter on "Power." The headings, however, for continuing the chapter were found by themselves, and are given below:—

"Sources of mechanical power, of two kinds, may be considered—

(*a.*) Those which work on the spot.
(*b.*) Reservoirs of Force.

(*a.*) Divisible into—
1. Those that work by expansion.
2. " " contraction.

1 A. Heat applicable to—
α. Fluids.
β. Liquids.
γ. Solids.
α. Air Engines.
Benzole.—Explosive.
β. Steam.
Other Vapours.
Liquids which expand.
γ. Expansion of Metals.
1 B. Freezing Water.
1 C. Wood-absorbing Water.
1 D. Sodium and Water.
} Those that work by expansion.

2. α. Electro-magnetism.
β. Moistened Rope.
} By contraction.

(*b.*) Reservoirs of Force divisible into virtually Springs. To find the best spring:—
α. Mechanical.
1. Air will not do.
2. Carbonic Acid.
3. Nitrous Oxide.
4. Caoutchouc. Steel.
} Tides.
Winds.
Balloons.
Gravity.
β. Chemical.
Gunpowder moistened.
Gun-cotton.
γ. Vital.
Human Muscle magnetised."

In the Appendix we find more than three pages of titles of books or memoirs consulted by the author in the composition of his work. From beginning to end of the book we are struck by the fact that the author has consulted every available source on the subject before committing himself to an opinion or offering a hint as to a more perfect method. The work must always be of value, if not for its suggestions, at least for the clear logical manner in which the *pros* and *cons* are placed before the reader. But we claim more for it than this: we think the author has treated the subject in an eminently suggestive and often original manner, and we cordially recommend the book not only to the rapidly-increasing list of members of the Aëronautical Society, but to all readers interested in a subject which has puzzled mankind since the time of Dædalus.

A Classified English Vocabulary, being an attempt to facilitate a Knowledge of Words and their Meanings by an Arrangement of Ideas according to their Scientific Connection. London: Provost and Co.

This little book is absolutely anonymous, not bearing on its title-page even a *nom de plume*. From the Preface we gather that the fundamental conception of the work first suggested itself to the author whilst attending a lecture on Mnemonics, in which was illustrated the singular facility with which a long string of names could be remembered when every name was linked to the name which followed it by some observed connection between the ideas conveyed by them. We further learn that the construction of the classification adopted has been "very much facilitated by the guidance derived from the classification of ideas comprised in Dr. Roget's 'Thesaurus.'"

In attempting to form a conclusion on the merits of the treatise, a distinction must be drawn between the value of the author's fundamental conception and the manner in which it has been here carried out. We fully grant that not merely in daily life, but in public speaking, and even in writing, there is a great —and we fear an increasing—vagueness in the use of terms. Men do in fact "connect with the name no precise knowledge of the things, but apply it rightly or wrongly to every object which seems to possess this undefined sort of resemblance to various other things called by it." To give a familiar instance—Does not all England outside of strictly scientific circles persist in calling the cockroach a "black beetle," although it is no beetle at all, but a congener of the locust and the cricket? And, on the other hand, do not our American cousins call the new insect-scourge which is travelling eastwards from Colorado, and which is a true beetle, the "potato-bug?"

We fully agree with the author that such vague uses of language are a serious evil, and that any person by giving way to it tends to form "defective habits of observation" and a general looseness of thought.

But having admitted so much, the question remains—What is the remedy for this vagueness? On this point let us hear what the author says in his Introduction :—"As signs serving to denote an indefinite number of resembling individuals, to identify species under genera, and genera of a lower grade under genera of a higher grade, the general names of a language may be considered not only as instruments of thought and of communication, but also as originally being in themselves the clue to an important body of knowledge. For every general name expresses certain attributes, and the similar things which it denotes are all things whatever which possess these attributes. In learning, therefore, to apply the name to various things possessing the attributes which it expresses, the learner will be at the same time gathering knowledge of the things themselves, as agreeing with one another and differing from other things in certain definite respects. In the case, therefore, of a well-developed language, or one embracing the whole universe of discovered or imagined things under various degrees of generality, its gradual acquirement by any one as his native tongue would appear to involve a vast accession of knowledge, a certain insight indeed into the nature of all things known. Attainment of this knowledge, moreover, would seem to involve as a result a mind made open to every variety of impression ; an intellect quick, through constant exercise, to observe, compare, discriminate, and identify,—to perceive differences between things which resemble, and points of agreement between things the most seemingly different from one another."

To us this passage seems fraught with dangerous error. To endeavour to acquire a knowledge of things by comparing, analysing, and classifying the names by which such things happen to be known, was a characteristic feature of the so-called "stationary period" of the human mind, and was one of the chief reasons of the impotence of the philosophies which prevailed before the epoch of Bacon, Descartes, and Galileo. No amount of inquiry into or speculation on words will enable us to discern minute differences between things which resemble each other, or occult points of approach between things seemingly distinct. For this purpose we must go to the things themselves. It is agreed by all competent judges that the mere reader, or the mere hearer, the man of words, however carefully he may weigh and define the terms which he uses or meets with, will have merely a dim and—so to speak—hearsay knowledge of the subject which he takes up. If we wish to know the properties of matter or of force, we must work with or upon it. That very vagueness of which our author justly complains is mainly due to the circumstance that in our ordinary educational training thing are left out of sight, and words are treated as if they were not counters, but money.*

If, to return to the illustration which we used, it is desired that the words "beetle" and "bug" should be used in an accurate manner, surely the best method is to point out to the pupil the distinctions between the two classes of insects denoted by these terms. If this is once done there will be no more mistakes of the same kind. We should also fear that our author's scheme, if used without the correction of a frequent appeal from words to the things which they represent, would rather tend to cherish the belief—already too common—in a "pigeon-hole" arrangement of the world. In physical science and in the affairs of daily life we have continually to classify; but our genera, groups, or whatever we may call them, are established not so much by reference to a definition, or outside boundary wall, as to a type within.

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* "Words are the counters of wise men, but the money of fools."

It must not be supposed, however, that we condemn the author's system in its totality. On the contrary, subject to the control of a constant appeal, whenever practicable, from words to things, from the symbol to the object symbolised, we think the general features of his plan might be advantageously introduced in vocabularies. Only let no exaggerated expectations be formed as to the nature or the kind of knowledge and of mental discipline that may be expected to result from its introduction.

As to the execution of the plan, we do not consider that the author's categories are altogether beyond the reach of criticism. Errors are not wanting. Let us, for instance, turn to "Class VI. Persons. Sub-class I. Persons in relation to Intellect." Here we find a distinction between "No. 369, *Thinker*, and No. 370, *Reasoner*," the former head being made to include the speculator, inquirer, examiner, investigator, explorer, censor, and critic. Now it is, we submit, impossible to speculate, examine, or criticise without reasoning, and hence we fail to see how the author's classification can be maintained. A little further we find "sceptic" introduced under the head "unbeliever." This may be in accordance with the conventional usage of the tea-tables and of "serious society," but in strictness a sceptic is one who neither affirms nor denies, but suspends judgment. Under the class "Dunce" figure the "sciolist" and the "smatterer." We wish the author had introduced the "good man all round" of modern examinations, who is incomparably the most hopeless specimen of duncedom ever produced.

Again, we find under "Class V. Things arbitrarily distinguished, constructed, or produced," Sub-class IV. Food. No. 331. Alcoholic liquors, ale, beer, porter, cider, and perry. But wine is found not along with these kindred fluids, but under "Class II. The material world. Sub-class VI. Organic nature. Section IV. Vegetable products. No. 132. Starch, sugar, and allied or derived substances." Why the juices of two fruits, pressed out and allowed to ferment, should be assigned respectively to two such distinct places in the author's classification we must confess ourselves unable to understand. Nor yet can we see why arrowroot, sago, and tapioca should be banished from the class "Food." Now, a classification which admits of things respectively closely allied being placed under different heads must be rejected as essentially faulty.

Wine and its Counterfeits. By J. L. DENMAN. London: 20, Piccadilly.

THE author of this interesting little pamphlet is well known for his advocacy of the merits of the pure wines of France, Hungary, and Greece, against the depraved public taste in favour of the mixtures known as port and sherry. Mr. Denman has done more than write; he has spent over ten years in making known the wines of the Greek Archipelago and Islands—a work, doubtless, far from profitable for the first few years, owing to having to fight against old and well-established prejudices.

The work contains several interesting tables relating to wine, valuable extracts from the researches of Dr. Thudichum, Dr. Dupré, Dr. Barry, Dr. Edward Smith, Dr. Faure, and many other eminent authorities.

The admirers of sherry will feel much comforted when they read the process by which their favourite wine is made, and how the wholesome tartrates and malates are converted into sulphate of potass by the operation of plastering: so successful is the result, and so rich in this salt are some samples of sherry, that it is rather a matter of surprise that it has not long ago taken its place in the Pharmacopœia as a purgative medicine.

Port is scarcely better: the finest samples may be very good as cordials, to be taken as cautiously as Curacoa and Maraschino, but they are certainly not wine, and should never be sold as such by any merchant who values his reputation as a dealer in pure and wholesome wine. There is not the slightest doubt that many of these mixtures come within the scope of the very unsatisfactory Adulteration Act, as fraudulent mixtures of a nature injurious to health. Gout especially is one of the diseases attributable to fine old port, a concoction for which the British nation has to thank that late eminent statesman William Pitt.

Wine analysis demands very much the attention of our chemists; some means of distinguishing between alcohol, the direct product of fermentation, and distilled alcohol is especially needed; they are two very different substances, varying greatly in their action, as anyone may try for himself by noting the effects of a glass of good claret, or other unfortified wine, and an equal quantity of brandy or whiskey diluted to a similar strength.

The chapter, "How to know Pure Wine," is not the least valuable part of this little pamphlet, and is so useful that we quote it at length :—

"1. That pure sweet wine is of low alcoholic strength.

"2. That all perfectly fermented wine is dry, and of high alcoholic strength (varying according to circumstances, and then rarely exceeding 26 to 28 degrees of proof spirit), and is not sweet, as all the sugar from the grape has been converted into alcohol, and should have when young an acid, or rather sub-acid, taste (not acetous), from the presence of tartaric acid, which is the natural and healthful acid appertaining only to wine. Its removal, by gypsum or plaster of paris, converts the tartrate of potass into sulphate of potass, which is a purgative and bitter salt, with a depressing action on the heart. All sherries are plastered.

"3. That the addition of alcohol to wine, either before or after fermentation, renders it unwholesome, and conduces to gout and similar disorders.

"4. That the greater the amount of *natural* alcohol produced in wine the greater is the amount of body in it, as the other constituents of wine must have been produced *pari passu*, and have been existent in the grape to yield the amount of alcohol, whereas added spirit does not give body.

"5. That the greatest amount of natural alcohol in wine is produced in those climates in which the grape attains the greatest perfection, and consequently contains the largest amount of sugar, combined with the other constituents of the fruit.

"6. That as all port, sherry, Madeira, Marsala, Catalonian, and Roussillon contain from 36 to 42 per cent of spirit, they have been either checked in the fermentation by the addition of alcohol to retain the sweetness of the must, or, after the fermentation was completed, the wine must have been sweetened and spirited to bring it up to the regulation standard. Furthermore, that added spirit causes an undue deposition of the tartrates and neutral salts of the wine (thereby depriving the wine of that life, freshness, and character which render it so valuable as a remedial agent), covers defects, and enables all sorts of mixtures to be made up and sold as port, sherry, &c.

"7. That the addition of alcohol to wine renders it of less pecuniary value, as spirit costs about 1s. 8d. per proof gallon without the duty, which is much less than the cost of wine : it therefore follows that, if it were not for the Excise and Customs Duty, spirit, the strength of ordinary port and sherry, could be sold for 2d. per bottle.

"8. That all natural wines, if any improvement is to be effected by age, must throw down a deposit, and thereby become *sweeter in bottle, by the elimination of their tannin, tartrates, &c.* From red wine the deposit contains tannin, which, uniting with the albuminous matter contained in the wine, forms a crust, that year by year becomes less and less, until at length it becomes so thin that it acquires the name of 'bee's wing.' The deposit also takes the form of crystals, which will both adhere to the cork and fall to the bottom of the bottle like powdered glass. All natural wines that have been any length of time in bottle should therefore be decanted with care."

Description Physique de la République Argentine. Par le Dr. H. BURMEISTER, Directeur du Museo Publico de Buenos-Ayres. Traduite de l'Allemand par E. MAUPAS. Tome Premier, contenant l'histoire de la découverte et la géographie du pays. Paris: F. Savy. 1876.

WE have before us the first volume of an elaborate and important work, which Dr. Burmeister contemplates issuing on the Argentine Republic, and in which will be embodied the results of twenty years' careful study. It is intended that the first two volumes should form the introduction, as it were, to the main body of the work. the scope of which, as a whole, will be best and most concisely described in Dr. Burmeister's own words:— "Je n'ai pas à fournir," he remarks, "un traité de géographie de la république Argentine, et encore moins une description de sa richesse minéralogique; mais il me suffira de faire connaître dans leurs généralités le sol, et le milieu dans lequel, vivent ou ont vécu dans les temps préhistoriques les animaux et les plantes qui seront étudiés spécialement dans les volumes suivants. L'ouvrage sera surtout consacré à ces deux règnes, et son but est de donner un tableau des diversités organiques de ces deux groupes, tableau qui commencera par le règne animal. Le règne végétal et la description géologique du sol de la république ont étés confiés à de jeunes savants qui publieront leurs divers travaux sous leur nom personnel, comme parties de l'œuvre totale."

It is naturally impossible to form a decided opinion as to the merits of this work from this the first volume, but Dr. Burmeister's reputation as a geographer will be a good guarantee for the correctness of his facts and the soundness of the views expressed; and it will be sufficient for us to observe that the present instalment of the book is devoted to a history of the discovery and of the early colonisation of the country, and, secondly, to a sketch of the geography of the republic, in which the author has availed himself of special materials to be found only at Buenos Ayres. It may not be uninteresting to add that the work is published in German and French, at the expense of the Republic.

The Empire of Brazil at the Universal Exhibition of 1876 *in Philadelphia.* Rio de Janeiro: Typographia e Lithographia do Imperial Instituto Artistico. 1876.

THE only works hitherto published which afford very complete information respecting the Empire of Brazil are the "Breve Noticia" and "O Imperio do Brazil," prepared for the Paris and Vienna Exhibitions of 1867 and 1873. In the interval which has elapsed since the latter publication was issued Brazil has in some respects improved her position among the nations of the earth, and the compilers of the works referred to have thought it due to "the old and constant friendship which links the two countries," no less than the important commercial relations existing between them, to issue the volume under notice for the Centennial Exhibition at Philadelphia.

The work deals with almost every conceivable subject, and supplies ample information on all; but, perhaps, the chapters which will be read with most interest are those which treat of the animal, vegetable, and mineral kingdoms, the last-named being, on the whole, the most attractive. The compilers have wisely published the result of their labours in English, and, considering the great disadvantages with which they must have had to contend, we are bound to admit that the mistakes in spelling and diction are much fewer than might have been expected.

Rain and Rivers, or Hutton and Playfair against Lyell and All Comers. By Colonel GEORGE GREENWOOD. London: Longmans and Co.

THIS somewhat fantastic title introduces to us a work which may perhaps be regarded as a contribution to geological science, or perhaps as an attempt to annul the larger portion of that science, and to render its reconstruction, in anything like its present extent and importance, an impossibility. Colonel Greenwood's direct and immediate purpose is to show, in opposition to Lyell, that valleys have not been formed by the action of oceanic currents, waves, &c., prior to or during the elevation of the land, but are of atmospheric origin, having been gradually excavated by "rain and rivers." For this view scientific evidence is adduced. But the author maintains other propositions, no less zealously, though with much less foundation in facts or in sound argument. Thus he tells us that "man and the mammalia—that is, the most perfect creatures—may have existed on the land from the beginning, and *before* the first strata were formed in the sea, and consequently that Darwinism and the development-theory are myths. The land is the region of perpetual disintegration, denudation, and destruction. The sea is the region of perpetual deposit and conservation. And sea-strata when hoisted up by heat in the form of slate, sandstone, limestone, &c., form museums of sea-life of an antiquity quite incomprehensible to man. But these museums contain sea-life only: land-animals neither live nor die in the sea; they live and die on the land. And where are the museums for the preservation of ancient land-life? There cannot be such a thing. The entire surface of the earth is perpetually vanishing, and with it the museums for the preservation of ancient land-life. The most ancient museums of land-life are caverns, filled-up lakes, bogs, and drift and alluvium—things which, geologically speaking, were formed yesterday and will be gone to-morrow. In these modern land-museums, however, the remains of man and extinct mammalia are found, and they *would* be found in more ancient land-museums if such ancient land-museums *could* exist. . . . . The whole affair is the result of the most childish confusion between space and time, between place and period. That is, because in the deposit of a certain *place* such a life only existed, we set it down that in the period when that deposit was formed that life only existed. Hence such errors as 'The age of reptiles,' 'The diluvial period,' 'The boulder period,' 'The drift period,' 'The period of invertebrates,' 'The pluvial period,' 'The gravel period,' 'The peat period,' &c." At the same time, however, the author admits that species have died out and that others have been successively created, "and at distinct times, and in comparatively modern times." Further we read—"Unless we except man (as I think we may) the existence of all organic species is not only finite, but it is transitory as compared with the existence of the globe, and it depends on second causes." Therefore the whole doctrine of the successive appearance and disappearance of species, which we have just seen denied, is after all admitted! This appearance and existence, too, is in one place spoken of as due to "second causes," and yet in another it is referred to a succession of creations!

All the knots of animal and vegetal geography are abruptly and compendiously cut. "Like the chicken-fancier who keeps his fowl-yards separate, Nature seems purposely to have contrived *different* stations with *similar* physical conditions, in order to exhibit the profuseness of her creative power in cramming all full of animal and vegetal existences with constitutions similar to those of similar but separate stations, but the species of each similar separate station differing entirely from the species of all other separate stations." We need scarcely point out how radically unscientific, or rather anti-scientific, is the spirit of this passage. If this method of accounting for the phenomena which meet us in the universe is to be accepted, why should we observe, register, classify, or seek to account for anything? Why not close our books, give our apparatus to the children for toys, and say "things are because they are," which is merely a plainer and briefer way of expressing our author's views. We will venture to say that had he even devoted a single year to the serious study of animal geography, he would have been more chary of referring to "Nature's intentions," and would never have sought to account for the distribution of species in such a manner.

Various other utterances, more remarkable and well founded, might easily be selected, if any useful purpose could be thereby answered.

It is to be profoundly regretted that one who displays so much rashness in theorising, and who reasons in such a very peculiar manner, should not have deemed it incumbent upon him to show a little more courtesy toward those from whom he dissents. Such expressions as the "pompous Humboldt," whose "ideas appear like the ravings of madness," are happily not customary weapons in scientific controversy, and must assuredly damage the man who has the questionable taste to use them much more than the one at whose memory they are levelled.

All that is really valuable in this book might well have been compressed into much smaller compass, and certain of the sections ought never to have been written.

Hay-Fever or Summer Catarrh; its Nature and Treatment. By G. M. Beard, A.M., M.D. New York: Harper and Brothers.

We cannot presume to endorse the opinion of an old and somewhat testy friend, that "hay-fever is all humbug," although certainly we have never met with or heard of a case in private life, and know of its existence merely from medical works. Though not recognised as a distinct disease prior to 1819, it seems to have become prevalent in England, and still more in the northern part of the United States, and already attracts a considerable amount of attention. Unlike his predecessors in the enquiry as to its nature and origin, Dr. Beard holds that hay-fever is essentially a neurosis—a functional disease of the nervous system. "The debilitating influences of heat and the external irritation of vegetable and other substances (pollen from different plants, essential oils, &c.) are exciting causes merely." He does not believe in the existence of any specific suitable for all persons suffering from the disease.

It is interesting to note that in America a special Association has been formed for the investigation of this affection.

*The Solution of the Most Important hitherto Unsolved Problems in Nature.** By J. F. Lochner. Cöln and Leipsig: E. H. Mayer.

Germany, fruitful as she is in sound scientific research, is not very rich in scientific heresies: she produces few attempts to set aside at a stroke the labours of our most illustrious philosophers, and to solve questions which they have found too difficult. She has, we believe, no "Zetetic Astronomy." no "Trinology," no "Origin of Creation." Some persons may ascribe the absence of such intellectual vagaries to a want of independent thought and a tendency to accept existing systems without challenge. We should rather seek for an explanation in the circumstance that in Germany public opinion does not encourage a man in coming forward to enlighten the world on any matter without having made it the subject of his thorough and especial study.

The work before us seems an exception. The author appears equally able to deal with unsolved problems in astronomy, physics, chemistry, biology, or geology. As a specimen of Herr Lochner's method we will take his reply to the question why the surface of our globe "consists of one-quarter land and nearly three-quarters water, while the inverse proportion would be certainly more suitable?"

To the common view that this excess of water is required to supply sufficient moisture for the support of the vegetable world, he objects that of the ascending vapours about two-thirds must fall back uselessly into the sea from which they have arisen. He further contends that "there are districts where it rains very seldom or not at all, and where everything yet flourishes. Here the vapours fall down in the night as a strong dew." But does not the author see that an abundant supply of vapour is equally needed whether the earth is to be watered by rain, dew, or by rivers? His view as to the use of the ocean is that it constitutes a kind of reserve, from which, when the human race becomes more numerous, and when even "this great America" is overpeopled, new islands and even new continents will rise up. Now, that islands have risen up and that continents have become extended appears to be beyond dispute; but unfortunately islands, and even continents, are considered, on equally good evidence, to have disappeared. What is worst of all, the lands that have been thus lost would seem to have been much more valuable and better adapted for the habitation of man than those which have been elevated in their stead. Thus Siberia is doubtless a wretched compensation for the continent that is supposed to have extended eastwards from Australia. Whether the relative proportions of land and water remain unaltered, or whether the solid portion of the surface tends to increase, we are unable to decide. We must remember that the sea is no less replete with organic life than the dry land, and if we could contract it we should merely give greater scope to certain living forms at the expense of others.

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* Die Lösung der wichtigsten bis jetzt noch unerklärten Probleme in der Natur.

In another chapter we read—" I hold therefore that the gravitative force is the finest that exists in all creation, as it consists of the ultimate atoms conceivable in space. As this matter has no intervals or pores, it can consequently contain no heat-matter. But whatever in nature contains no heat must have cold,—consequently gravitation is absolutely cold."

In short, the author—as he states in other passages—regards cold not as a mere degree of heat, low with regard to our feelings or to the ordinary temperature of objects around us, but as something actually existing in nature. This is one of the leading doctrines of Forfar's " Trinology." Nitrogen, the author considers, if compressed by a sufficient degree of cold, is changed into *infinitely small particles of iron !* " From the highest strata of the atmosphere in which this took place these particles fell, by reason of their gravity, to the lowest, and thus became diffused over the whole surface of our globe. It is therefore not to be wondered at if iron is to be found everywhere." With this sentence we will conclude our notice of this extraordinary work, leaving our readers to meditate on this new theory of the origin of iron.

Statistical Investigations on Mental Diseases.[*] By F. W. Hagen. Erlangen : Eduard Besold.

We have here an application of the statistical method to the solution of certain questions connected with the painful subject of mental alienation. After a very able Introduction, treating of medical statistics in general and of the statistics of this class of disease in particular, the author gives a short account of the asylum for central Franconia. He then discusses the influence of the duration of mental disease previous to the admission of a patient into the asylum, upon its total duration, the general mortality of the insane, the various forms of the disease, and the influence of sex, age, and civil status.[†] From these topics he passes to the capital point, the heredity of insanity. Here his observations lead him to a result which he rightly terms " consolatory," namely, that " the inheritance of psychic disease does not occur so frequently and so unconditionally as might be supposed from the impression made by affirmative instances." The number of patients in whom a hereditary tendency could be traced was to the total number as 1 : 2·68. In the male sex alone this proportion falls to 1 : 3·05, but in the female rises to 1 : 2·34. Acute derangement is more frequently hereditary than chronic mental affections to the extent of 7 per cent. In acute, chronic, and paralytic lunacy the paternal influence is to the maternal as 100 : 103·3. Hence a tendency to mental affections in the female line is somewhat the more formidable. Parents whose madness is not hereditary have more frequently sound children than those whose disease has been inherited, and the offspring of the latter are more frequently weak-minded or idiotic. Cases where all the children die young are more frequent where the madness of the parents has not been inherited. In case of lunatic fathers the children were more liable to mental disease than the grandchildren. In case of lunatic mothers the rule is reversed. Moral degeneration, on the other hand, increases in the second generation. The only doubt capable of affecting the author's conclusions is that hereditary tendency to disease may often exist where it cannot be traced. Very few persons could show that not merely all their direct ancestors for some three or four generations back, but all the collateral branches of the family in the ascending line, were perfectly free from insanity. Indeed such an inquiry could not possibly be undertaken with success unless we were first provided with a rigid definition of insanity, and were able to say where "oddness," "eccentricity," "peculiarity"—perhaps even where genius—ends and where madness begins. There are grounds not to be altogether neglected for considering genius as a morbid product, bearing a closer semblance to insanity than is commonly dreamt of.

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[*] Statistische Untersuchungen über Geistes krankheiten.

[†] The word which we, for want of a better term, translate by " civil status " does not refer to rank or occupation, but to the distinction between the unmarried, the married, and the widowed.

Another long and ably-conducted investigation is devoted to the connection between insanity and phthisis. The author finds that the insane succumb to pulmonary consumption in a fivefold higher ratio than the sane, and that, conversely, among those affected with tubercular degeneration, mental disease is five times more common than among the non-tuberculous. In a largely preponderating number of cases the mental disease precedes the pulmonary affection. Hence pulmonary consumption cannot be considered as the cause, but rather as a possible effect of insanity.

We cannot too strongly recommend this thorough-going, masterly, and truly philosophical treatise to medical men, and to all who have any interest in this important subject. We must, however, point out that the value of the book would have been greatly enhanced by a more copious table of contents, or by a good index.

Peabody Academy of Science. Fourth Memoir. *Fresh-water Shell-Mounds of the St. John's River, Florida.* By JEFFRIES WYMAN. Salem, Mass.: published by the Peabody Academy of Science.

ZEALOUS and praiseworthy attempts are being made in America to ascertain the history of the tribes who once occupied the Western Continent, but who have faded away, either since the arrival of European settlers or in far earlier periods. Much important evidence it is to be feared has passed hopelessly away. The ignorant bigotry of the earlier Spanish settlers destroyed the most important monuments and inscriptions, and the sloth of their successors has allowed much more to perish. But much may be done by a diligent use of what remains, and we may yet learn no little of the nature and habits of nations who inhabited North America thousands of years ago, and who at first sight appear to have died and left no sign.

"Shell-mounds" are very widely distributed. In a certain stage of culture—or rather of the absence of culture—mankind seem to have established themselves on the shores of the sea and of certain lakes, and to have subsisted mainly upon "shell-fish." This was undoubtedly before the discovery of suitable tools had rendered agriculture possible—probably before weapons for the chase were invented. The peninsula of Florida contains numerous of this class, both on the sea-coast and along the banks of rivers, composed of a mixture of shells and bones of animals which have been used as food. These mounds contain fireplaces, and various tools made of stone, bone, or shell. The stone implements are rarely met with in the mounds themselves, but are more abundant on the surface, and are of a very rude character. The bone and shell tools are of superior workmanship, but are believed not to have been made by the tribes who formed these heaps, but to have been introduced by immigrants from the north. In the more recent mounds, though not in the oldest, fragments of pottery of a very rude kind have been discovered. Evidences of the prevalence of cannibalism are unmistakable, as bones of human beings are found, broken up in the same manner as those of other animals. Bones and teeth of extinct animals are found,—such as the mastodon, elephant, horse, and ox, but these have undergone changes which show that they were not contemporary with the builders of the mounds. It may be approximately ascertained that some of these mounds were finished at least two or three centuries before the arrival of the first European settlers. The balance of evidence inclines to show that the formers of these mounds were not the people found in Florida by the Spaniards and the French, but a more primitive race, having apparently no knowledge of agriculture.

Elementary Arithmetic, with Brief Notices of its History. By R. Potts, M.A. Cambridge: W. Metcalfe and Son. London: the National Society's Depository.

This work differs strikingly in some respects from the ordinary arithmetical treatises used in schools. The first of the twelve sections of which the work consists is devoted to an introductory dissertation on number, on numerals, and the history of arithmetic, and contains much curious and interesting information. The second section treats on money, and gives a history of the British coinage from Tasciovanus, king of the Trinobantes (B.C. 30 to A.D. 5), down to the present day. Section III. is devoted to the consideration of weights and measures. The writer is evidently hostile to the metric system, and we may venture to say that one of the main objects of the work before us is to delay or altogether prevent its introduction into England. We regret that such a spirit should find scope in a work which, if we mistake not, is adopted as a text-book in the "National" schools of England. What advantages are to follow from the retention of our present involved and cumbrous system the author does not show, and we cannot help suspecting that, both on his part and on that of the three eminent men of science who share his opinions, prejudice—perhaps scarcely conscious—is in reality the moving power. With many very well meaning but not over clear-headed people we know that the metric system is in some mysterious way associated with atheism, Robespierre, and the reign of terror. The proposal to abolish the so-called "compound" rules of arithmetic, and to make all addition simple addition, is supposed to involve a covert plot against the altar and the throne. Section IV. treats of time and its divisions, Section V. of logarithms, and it is not until Section VI. that the consideration of the ordinary rules of arithmetic is taken up.

Were it not for the attack upon the metric system we should feel bound to give the work our cordial recommendation, and as it is we must bear witness to the clearness with which the author unfolds every successive portion of his subject, and aims very successfully at rendering each step thoroughly intelligible to the student.

Monthly Notices of the Papers and Proceedings of the Royal Society of Tasmania, for 1874. Tasmania: Mercury Office, Hobart Town.

The amount of valuable work which may be done by our colonial scientific societies is immense. To them the learned world must look for detailed and accurate accounts of the fauna and flora, both fossil and recent, of interesting regions concerning which our knowledge is a mere sketch, even if accurate. We observe that the Society is engaged with the formation of a museum, which we hope will become rich in local collections in all departments. This is the more important as many plants and animals in the colonies may soon become extirpated, owing, on the one hand, to the wanton destruction perpetrated by ignorant men, and on the other to the importation of aggressive plants and destructive animals. The Society pays also considerable attention to the acclimatisation of products which may be of commercial value.

Among the papers communicated we may point out as particularly interesting one by the Rev. J. E. Tenison Woods, F.L.S. and F.G.S., on the "Physical and Zoological Relations between Australia and Tasmania." The author compares especially the eastern portion of the Australian continent with the island. He considers that Tasmania unites, in her natural history, features which are characteristic of distinct provinces in Australia. Eastern Australia he divides into three zones;—The coast region, characterised by a genial humid climate, with a vegetation in the temperate regions of almost tropical luxuriance, and generally Asiatic in its features, becoming more decidedly so as we advance northwards. The second zone is the table-land with the mountains, having a distinct vegetation, alpine in the southern portions, and in the northern more essentially Australian and less Asiatic than the coast regions in the same latitude. The innermost or desert zone extends to the south coast, except where mountains intervene. It is more characteristically and peculiarly Australian in its features than the other zones, and possesses fewer forms common to other countries. The two first-mentioned regions are well represented in Tasmania, with modifications which tend to show that its separation from the mainland is not of very recent origin. The climate is much affected by the gap of Bass Straits, and the vegetation is much less luxuriant and less tropical in character. Its affinities seem to point rather to Polynesia or New Zealand than to Asia. The large and beautiful diurnal Lepidoptera common in Eastern Australia are either rare or totally unknown in Tasmania. The land-shells of Tasmania are all strongly-marked species, with very little affinity with those of the East coast, but having analogues in New Zealand.

This volume also contains a catalogue of the plants of Tasmania, drawn up by Baron Mueller. About fifty species have been discovered since the completion of Dr. Hooker's great work in 1860.

Abstract of Results of a Study of the Genera Geomys *and* Thomomys, *with Addenda on the Osteology of the Geomyidæ and on the Habits of* Geomys Tuza. By Dr. Elliott Coues, U.S. Army. Washington: Government Printing-Office.

A valuable monograph of two nearly allied genera of rodents. It is to be regretted that the author, in his paper on the habits of *Geomys Tuza*, has retained for it the vulgar and misleading name of "salamander," by which it appears to be known in Florida and Georgia. The animal is very injurious, undermining the heaps of potatoes buried in the fields, and carrying away the store.

United States Geological Survey of the Territories. A Monograph of the Geometrid Moths or Phalænidæ of the United States. By A. S. Packard, Jun., M.D. Washington: Government Printing-Office.

We always feel the most sincere respect for the man who has the courage and the wisdom to devote himself to the investigation of some one scientific question, however apparently limited, with the determination to ascertain all that can be known on the subject. A monograph may, indeed, be a less ready way to reputation than a popular manual, or a brilliant but crude and superficial generalisation. But it is a far more valuable contribution to human knowledge.

There may be persons so blinded by considerations of immediate utility, and so ignorant of the forces at work in this world of ours, as to sneer at the study of insects, and to feel aghast at a goodly quarto devoted to an exposition of the structure, classification, and geographical distribution of one particular class of moths in one section of the western hemisphere. We must, however, remind such persons that of all our enemies in the animal kingdom insects are the most formidable,—capable, indeed, of making life unendurable, of destroying the crops of whole provinces, and of exposing nations to the horrors of famine. To contend with such opponents of our well-being we must possess a full and accurate knowledge of their structure, constitution, and habits. Thus, even on the lowest utilitarian ground, Entomology is one of the most important of studies. This truth has been fully recognised in the United States, where fearful ravages have been committed by various insect-pests, and where, in consequence, both the Federal Government and the State legislatures have been aroused to the necessity of action.

The reader, if not an entomologist, may ask what is a geometrid moth? If he will observe caterpillars in the fields and woods he will find that some of them have, to speak popularly, an almost uninterrupted series of feet, sixteen in number, from head to tail, and when creeping always keep their bodies in close contact with the leaf or branch upon which they are supported. Others, of which the well-known gooseberry caterpillar (*Abraxas grossulariata*) have merely ten feet, arranged in two groups, one at the anterior and the other at the posterior extremity. When crawling they grasp the leaf or other object firmly with their fore feet, and then bring the hind feet close up to the former, so as to bend the body into a kind of loop, and thus advance by a kind of striding or spanning movement. The moths developed from such caterpillars are called Geometridæ: they are a very numerous family, though, not being remarkable either for size or from brilliant coloration, they are often overlooked. In Europe eight hundred species have been enumerated. In the work before us between three and four hundred species are described, but the author is so far from thinking his subject exhausted that he considers a thousand species may be found in the North-American continent. His region agrees with Mr. Sclater's Nearctic province,—in other words, all North America beyond the tropic of Cancer.

Commencing with a history of the family as characterised and subdivided by entomological authorities, he passes on to the comparative anatomy of the various groups, and thence to the description of species. The geographical distribution of the Geometridæ of the United States is finally considered. Unlike Mr. Sclater and Mr. Wallace, he subdivides the region not into four, but two provinces, the Arctic and the North Temperate Realm. Tables are given of the species peculiar to Greenland and Labrador; of the subarctic species common to Eastern and Western America, and to Europe and Asia; of the species inhabiting the eastern province of the north-temperate realm; of those found in the limits of the Alleghanian and Carolinian faunæ; of those occurring south of the isothermal of 60°, the territory corresponding to Le Conte's Southern province; and those of the Western province.

The author calls attention to two features of interest in the distribution of the insects of the Pacific slope, viz., the absence of forms characteristic of China and Japan, and the presence of some European types which do not occur in the Atlantic province,—features which may be traced in other Lepidopterous families, in other orders of insects. In order to illustrate the relations between the Geometridæ of different regions, he gives tables of the species (18 in number) common to temperate America and Europe, of the species common to the Eastern and Western provinces of North America, of those peculiar to North and South America, of those occurring in Central and South America—the Neotropical region, and of European genera not as yet discovered in the United States.

The following is an interesting contribution to our as yet very scanty knowledge of what may be called the local physiognomy of the faunæ of different countries:—"In all the species enumerated in the following list—25 in number—the Colorado examples (when the species have been found to occur there) and the Pacific Coast individuals are larger, and in some cases with longer and more pointed wings, than in those from Labrador and the Eastern Atlantic States, and in a few instances show a tendency to become lighter in colour."

The following passage is also worthy of special attention:—"It will be seen from the facts we have presented that the moths probably follow, as regards size, a law the reverse of that established by Prof. Baird for the birds and mammals, who shows that they decrease in size southwards, though his law of increase in the length of certain peripheral parts westward also obtains in the Lepidoptera. The increase in size westwards is of course equivalent to the well-known southward increase of size in insects, though in a few species of insects the Coloradan and Californian specimens are larger than Floridian and Texan insects of the same species. Of the insects mentioned in the list *Plusia Hockenwarthi* is the clearest example (1) of the law of increase in size westwards and southwards; (2) increase in length of peripheral parts; (3) brighter, deeper colours westward."

Dr. Packard is decidedly opposed to the theory of Heer, of an "inter-continental bridge between the temperate zones of America and Europe, and of America and Asia."

The work is admirably and abundantly illustrated: twelve large plates, with many hundred figures, represent not merely the moths as described, but the characteristic venation of their wings, and other important morphological details. Upon Dr. Packard the work reflects the highest credit. He has, evidently with a most lavish expenditure of time and labour, produced a most valuable contribution to zoological science, in which accuracy of detail and a large and philosophic spirit are equally conspicuous. Nor, whilst expressing our appreciation of the author, must we forget to commemorate the far-sighted and enlightened policy of the American Government in promoting such researches, and the more than royal munificence with which this and similar volumes are presented to learned societies, public libraries, and the conductors of scientific journals throughout the civilised world. Were this example followed by governments which have even greater opportunities, an absolutely priceless selection of materials would be placed within the reach of scientific men, and the verification of theories would be rendered a comparatively easy task.

The Sun—Ruler, Force, Light, and Life of the Planetary System. By RICHARD A. PROCTOR, Author of "Other Worlds than Ours," "Saturn and its System," "Light Science for Leisure Hours," &c. With nine Lithographic Plates (seven coloured), and one hundred Drawings on Wood. Third Edition. Longmans and Co. 1876.

THIS, as the Preface informs us, is a carefully revised new edition of Mr. Proctor's well-known treatise, with some important sections added, and from which the Appendix concerning the transit of Venus has been removed, as the purpose of that Essay has been accomplished.

We are glad that Mr. Proctor has thus re-issued his work on the Sun, and has not written a new book on the same subject with another title. In common with the majority of the most sincere admirers of Mr. Proctor's splendid abilities, we have watched his literary career with something akin to fear and trembling. So many new books following so closely one upon the other, with imminent risk of treading upon the heels of their predecessors, and mutually tripping each other, may ultimately overtask and dilute the energies even of Mr. Proctor's powerful intellect. It is therefore very satisfactory to find that he is working upon standard books as well as bookseller's books, or books for the season. This work with its companion treatises, "The Moon" and "Saturn and its System," will doubtless hold their ground as standard works on their respective subjects, and be followed, we hope, with corresponding standard treatises on the other members of the solar system.

Such works unfortunately are, at the outset, far less remunerative to the author than Mudie-books, that run for a season or two, and must be replaced by something that seems to be different, and must be, above all things, *new*, if only in title; but there can be no doubt that carefully-written and slow-selling standard works are ultimately, by virtue of their permanency, the most profitable, not only as regards reputation, but even as a source of income to the author, who, unlike the publisher, cannot take up another writer, and trade upon another reputation when his own is worked out.

The great merit of this work, as of the carefully-written portion of Mr. Proctor's other productions, is that it is popular without any sacrifice of scientific precision or soundness; and this popularity is attained without treating his readers as though they were babies, with any overwrought affectation of extreme simplicity. The facts and phenomena are plainly and directly described, without any excess of those "familiar illustrations" with which so many of our modern popular treatises are so thickly padded, to the confusion of the student and the exclusion of important information on the actual subject of the work.

Mr. Proctor's mathematical attainments have not deprived him of the use of unsophisticated language and argument; he can reason as well as formulate; he does not use mathematical expressions as a *substitute* for descriptive reasoning, but only when they are actually demanded by the essentially mathematical nature of his subject. Even then he uses the simplest forms, such as most ordinary readers can follow with a slight effort of attention.

The first chapter of this book, dealing with the strictly mathematical subject of the sun's distance and dimensions, is a notable example of this admirable characteristic of Mr. Proctor's dissertations; and similar powers of elucidation are displayed in the third chapter, on analysing sunlight, We know not where else the unlearned reader could find an equally profound yet clear and simple treatment of these subjects.

The interest of the later chapters is considerably enhanced by the author's free handling of the theoretical elements of the subjects. Scientific writers of the patronising school are too apt to assume that debatable hypotheses are above the reach of the populace whom they condescend to instruct. Mr. Proctor evidently proceeds on almost an opposite principle, and treats his readers with quite a banquet of speculative viands, leaving them to choose those that are the most agreeable to their own intellectual palates. This adds greatly to the interest of his books. Many who would fall asleep over a detail of bare physical facts and quantities are kept awake by the discussion of contending hypotheses, and facts which would otherwise slip from their memory are fixed there by their necessary association with an exciting controversy.

The absence of a full index is a sad defect in this book. The table of contents is good enough, fuller than usual, but this can never supersede the necessity for an alphabetical index to every standard work that is to be used for the purpose of obtaining solid information.

It may be that Mr. Proctor is a bad index maker, or too busy to attend to this mechanical department of authorship. If so, he would do well to call in the help of a friend who has the faculty of index-making. It is by no means necessary that the author should make his own index. The best index extant is that which was made by the late Robert Cox, of Edinburgh, for the "Encyclopædia Britannica:" and the next best with which we are acquainted is that appended to his own work on "Sabbath Laws and Sabbath Duties," a pamphlet of 16 pages with 547 pages of Appendix; the pamphlet in *pica*, the Appendix in *small pica*, and the voluminous notes to the Appendix in very close *brevier*.

Lessons in Electricity at the Royal Institution, 1875-76. By
JOHN TYNDALL, D.C.L., LL.D., F.R.S. London: Longmans
and Co. 1876.

THIS little volume contains, with slight modifications, the substance of Prof. Tyndall's Christmas Lectures given to a juvenile auditory. We have been accustomed to go to the Royal Institution in order not only to hear the newest and most original matter in the world of science, but to see the most finished and elaborate apparatus; to see the simplest things projected on the screen by means of the electric light, and to see the least wants and wishes of the lecturer anticipated by a host of ready assistants. But in the present instance it has been the aim of the lecturer to show how much can be done by means of the simplest and most inexpensive apparatus—a priced list of which is given at the end of the volume, the total amounting to £5 10s.

"I had heard doubts expressed," says Dr. Tyndall in the Preface, "as to the value of science-teaching in schools, and I had heard objections urged on the score of the expensiveness of apparatus. Both doubts and objections would, I considered, be most practically met by showing what could be done in the way of discipline and instruction, by experimental lessons involving the use of apparatus so simple and inexpensive as to be within everybody's reach." The Lectures are entirely confined to frictional electricity, and of this the book gives a very good elementary account, illustrated by many original and most ingenious experiments.

The contrivances are always of the simplest, and can be made by any ordinarily ingenious person: sticks of glass, sealing-wax, and gutta-percha, a lath balanced on an egg; a simple electroscope with Dutch metal leaves; eggs for conical conductors, and apples for spherical conductors: these are some of the appliances of which the author makes use. We notice on p. 28 a very ingenious experiment: a funnel with a stem of small bore is filled with fine silver sand, which runs out in a continuous stream; but if a wire proceeding from a rubbed glass tube be connected with the sand in the funnel, the particles of sand fly out from one another as they descend by self-repulsion. We strongly and cordially recommend this capital little treatise to all boys who possess a scientific turn of mind, and they cannot do better than make for themselves the simple apparatus necessary for the performance of the principal experiments in the book, and then work through the book during the long winter days of the Christmas holidays.

Elements of Physical Manipulation. By EDWARD C. PICKERING, Thayer Professor of Physics in the Massachusetts Institute of Technology. Part II. Macmillan. 1876.

THE first portion of this work was published three years ago, and comprised the Mechanics of Solids, Liquids, and Gases, Sound, and Light. The present volume completes the cycle of such work as may be performed in a physical laboratory, and includes Electricity, Heat, Mechanical Engineering, Meteorology, Practical Astronomy, Lantern Projections, and an Appendix containing various Tables.

A fourth of the work is given to Electricity. Commencing with a short description of batteries, keys, plugs, connections, switches, and commutators, he passes to the method of using them, and thence to the telegraph. The Morse telegraph only is described, and the account is somewhat meagre, but the testing of telegraphs and of submarine cables is discussed further on. Altogether, that part of the work devoted to electrical manipulation is somewhat disappointing, and it is quite insufficient for the present wants of students.

The section on Heat commences with an account of testing thermometers, and of the determination of the expansion of solids, liquids, and gases. The experiment (No. 127, p. 82) for determining the change of volume by fusion, as there described, is quite impracticable and impossible, and no student could obtain a satisfactory result by the use of it. Neither does the small accompanying woodcut make the matter much clearer. Under the head of pyrometers we find (in a space of two pages) an account of Wedgwood's pyrometer, the use of which has been long abandoned, also of the thermopile pyrometer, the pyrometer depending on the specific heat of platinum, and Siemens's pyrometer; but the accounts are so slight as to be useless without a considerable amount of collateral information obtained from some other source. The appliance recently described for illustrating the mechanical equivalent of heat as a lecture experiment is figured and described, but the name of the originator of the apparatus is not once mentioned.

The section on mechanical engineering is a novel feature in a work on physical manipulation. The author accounts for it as follows:—"Physical problems which are to be solved on a large scale require a thorough knowledge of mechanical engineering. To this class belong also those which have the greatest pecuniary value. The methods of conducting such experiments also are often so faulty that a brief description of how they should be performed will not seem out of place." The proper care of a boiler and of an engine is first discussed. The author points out that the loss of heat by condensation and radiation when steam passes through a great length of piping is much more considerable than is often supposed. By covering the pipes with canvass, or felt, or gypsum, the loss of heat may, to a great extent, be prevented, but the pipe of course takes a longer time to heat. A convenient form of friction break for directly determining the amount of work done by an engine is described on p. 126, and this is followed by an account of means of determining the speed of the piston-rod, fly-wheel, and shafting, and the friction of belts and pulleys. The space given to the strength of materials is small, but this branch of the subject is always treated of at length in books on applied mechanics.

Under the head of "Meteorology" an account is given not only of the determination of the more ordinary factors, but also of the three magnetic elements, and of the electricity of the air. For this latter determination Prof. Pickering proposes two instruments, one of which assumes the same electrical potential as the air, while the second measures this potential. This is usually effected by a Thomson's quadrant electrometer, or a Peltier if less accuracy is required. To collect the electricity an insulated vessel from which water slowly drops may be used, or a burning match made of blotting-paper dipped in nitrate of lead. During the progress of a distant thunder-storm the electrometer shows a sudden change of potential after each flash of lightning.

In the section on "Practical Astronomy" the author discusses the method of using the sextant for determining latitude, longitude, and time. Also time by the transit instrument, and latitude. A long account is given of the mode of using the equatorial telescope, and, finally, of the method of spectrum analysis applied to the stars.

A short section on "Lantern Projections" describes the various means of throwing photographs or images of apparatus upon a screen by means of an intense source of light, and a convex lens. As an apology for introducing the subject, the author remarks, "During the last ten years a new era has arisen in the illustration of lectures by the general introduction of the magic lantern as a means of demonstration. Not only in science, but in the mechanical arts, in architecture, and, in fact, in any subject susceptible of illustration by engravings or photographs, a few glass plates which may be carried in the hand will interest and instruct an audience more than the finest diagrams, which are, moreover, far more cumbrous and expensive. It is, therefore, desirable that every one who may have occasion to address an audience should be able to manage a lantern, and to project photographs on the screen. Again, especially in physical experiments, many objects are so minute that they cannot well be shown to a large number of persons, and an enlarged image of these may often be thrown on the screen, and thus be seen by hundreds at a time." The best methods of using sunlight, the electric light, the magnesium light, and the oxyhydrogen, or calcium light (as the author calls it) are described. For a lantern a wooden box about one foot in the side is recommended, furnished with various apertures capable of being closed by small sliding doors. Under the most favourable circumstances only about 12 per cent of the light emitted by the source can be made to fall on the screen. The author recommends the use of the screen as a blackboard; and he gives a rather interesting account of objects for projection. Designs may be rapidly drawn on blackened glass, or on films of gelatine. Models may also be well shown by projection if care be taken to make them lie in one plane. The author remarks that it is an interesting experiment to project the light itself upon the screen; and if this be the sun, sun spots may be easily shown by the means. He further observes that by this means "during a partial eclipse the phenomena may be watched by a large number at a time." But, surely, in the latter case it would be absurd to watch an eclipse in this manner in a room, when it is possible to see the eclipse and the accompanying phenomena in the open air. Again, chromatropes, kaleidoscopes, and the growth of crystals are common objects for the lantern. The vertical lantern is a useful appliance for projecting on a screen such things as magnetic curves. A lantern galvanometer is also described.

The final fifty pages of the book are devoted to Appendices; the first of which relates to Electricity, while the second gives a most useful set of tables, of Squares, Cubes, Reciprocals, Powers, Logarithms, Natural Sines, Natural Tangents, Logarithmic Sines, Logarithmic Tangents, Constants, Properties of the Metals, Properties of Liquids and Gases, Hydrometer Tables, Temperatures, Pressure of Vapours, Wet and Dry Bulb, the Solar System, Double Stars, Clusters and Nebulæ (with their Right Ascension and Declination). The constants are nearly all given in accordance with the metric system. Among the temperatures we find a cherry-red heat given as 900° C. (1650° F.), a yellow heat as 1200° C. (2200° F.), and a white heat as 1300° C. (2400° F.). It is much to be wished that new and accurate determinations of high temperatures were made by Siemens's electrical pyrometer.

The final Appendix contains some very useful hints regarding the cost and fitting up of a physical laboratory, which clearly prove that the cost of a physical laboratory ought not to much exceed that of a chemical laboratory, while the keeping up of it is certainly less, as there is a less consumption of material. A few pages (p. 296 *et seq.*) are well filled with a list of the most standard works of reference of each of the physical sciences; also of tables, catalogues, and periodicals. Finally, we have a list of a hundred additional experiments, of which the following are examples:—

"214. Measure the velocity of the bullet from a revolver, parlour rifle, crossbow, or catapult, with a ballistic pendulum.

If the catapult is used at all, attach it to the table, and determine the effect of drawing the spring by known amounts, and also by varying the weight of the ball."

"241. Devise a form of photometer for measuring the amount of light reflected at various angles by polished surfaces, in which a single light only shall be used."

"292. Measure the density of fog, or the amount of light absorbed by layers of various thicknesses."

At a time like the present, when physical laboratories are making their appearance at our universities, and even at some of our public schools, Prof. Pickering's book is a useful contribution to Science. It seems to us, however, to be far too meagre, and its value would be greatly enhanced by the introduction of really good woodcuts of the nature of those to be found in Deschanel. There can be no doubt that the volume will grow with each succeeding edition, and many improvements may be then introduced; meanwhile we recommend it to students who work in a physical laboratory as a really useful book, where they can apply for information concerning any part that is obscure to the professor in charge of the laboratory.

PROGRESS IN SCIENCE.

PHYSICS.

LIGHT.—Some useful experiments showing the action of Light on Ebonite have been described in "Nature," by Prof. McLeod, of the Royal Indian Engineering College, Cooper's Hill. It is, he says, well known to electricians that the insulating power of ebonite gradually diminishes, in consequence of the formation of a conducting layer of sulphuric acid on the surface; but it is not so well known that exposure to light facilitates this change, if indeed it is not an essential condition. He noticed that an ebonite plate electric machine which had been kept in a light room had changed in colour, except on those portions which had been protected from light by the rubbers. The exposed surface acquired a brown colour, and the machine acted very badly. On cleaning the plate with a hot solution of caustic soda, large quantities of ammonia were evolved, and the brown surface became softened, so that it could be easily scraped off. He therefore cut a plate of ebonite into four pieces, each about 52 m.m. long, 22 m.m. wide, and 8·5 m.m. thick, and one-half of each piece was varnished with an alcoholic solution of shellac. Two pieces were placed in test-tubes plugged with cotton-wool, the other two being sealed hermetically in similar tubes. One of each of these tubes were placed in a dark drawer, and the other pair exposed to light in the laboratory, and during the latter part of the experiment to direct sunlight. About nine months afterwards the tubes were opened, the ebonite washed with water, and the amount of acid determined by standard solution of caustic soda. No trace of acid could be detected on either of the pieces of ebonite which had been kept in the dark; on the one which had been exposed to light in the closed tube 0·343 m.grm. of sulphuric acid was found; and on the other, which for three weeks had been exposed to both light and air, 2·646 m.grms.

We learn, from the same journal, that Dr. Janssen is devising the construction of an automatic photographic revolver which will take a photograph of the sun every hour, each day of the year, from sunrise to sunset. The photographs which will be taken under cloudy conditions will be useless so far as sun-spots are concerned, but they might be utilised for meteorological purposes. The others will be kept and tabulated. The advantage of this plan is that it will dispense with any observer, and will obtain a mechanical regularity.

We have received from Mr. Todd, the Postmaster-General and Superintendent of Telegraphs and Government Astronomer of South Australia, a pamphlet entitled "South Australia, its Observatory and Meteorology." The Observatory is situated on the West Park lands, having the city of Adelaide and the Mount Lofty Ranges on the east, and St. Vincent's Gulf—towards which the land gently slopes—at a distance of about 5 miles on the west. Until recently its operations were chiefly confined to Meteorology, but advantage was taken of the transit of Venus in 1874 to procure a 10-feet equatorial by Cooke and Son, of York, and other instruments will shortly be added. Mr. Todd is anxious to turn the Observatory to account in the promotion of high-class education, by the delivery at the Observatory of lectures to students on Physical Science. In a recent Report to the Government he says that the Observatory is required not so much for the furtherance of astronomical science as for educational purposes. It will be able to render valuable aid in those fields of astronomical research which do not involve continuous observation or heavy computations—such work, for instance, as solar and stellar spectroscopy, sun-spots, double stars, and what Sir G. B. Airy has pointed out as a great want, observations of occultations, eclipses, and transits of Jupiter's satellites; but beyond this Mr. Todd is anxious to see the Observatory popularised as a School of Physical Science, at which regular courses of lectures should be delivered on practical and physical astronomy, navigation, meteorology, magnetism, electricity, heat, light, and optics.

Microscopy.—Prof. H. L. Smith, of Hobart College, Geneva, N.Y., makes use of the sheet wax of which artificial flowers are formed for mounting opaque objects, such as the Foraminiferæ, &c. Disks of suitable colour are punched out and pressed on a glass slide, which is warmed until the wax melts: when cool it will be found firmly attached to the slide, and the surface quite smooth and dead. A curtain-ring is pressed into the wax while warm, to form the cell-wall, and the whole finished off with Brunswick black outside in the usual manner. To attach the objects, a minute drop of turpentine is applied to the wax, and before it is quite dry the object is placed on the softened wax, and when thoroughly dry it will be found so strongly attached that a violent blow or fall will not dislodge it. While this is being done the Brunswick black on the ring will have set sufficiently to fasten the cover, which should be of such a size as to rest, not on the top of the ring, but to slip just within, so that its surface will be flush with the top of the ring. When the cover is pressed home the whole may at once, without any danger of running in, be finished with the black varnish.

A new process of histological staining has been communicated to the Quekett Microscopical Club, by Frances Elizabeth Hoggan, M.D. The tissues to be stained are membranes or soft sections, which may be either fresh, frozen, hardened in alcohol, or hardened by the picric acid and gum process; but such hardening agents as the chloride of gold, or any chromate whatsoever, are inadmissible. The colouring agents required are—a 1 or 2 per cent solution of perchloride of iron in distilled water or alcohol ("*tincture of steel*"), and a solution of similar strength of pyrogallic acid in water or alcohol, the latter fluid being preferable in both cases. The section or membrane to be stained is first to be treated for one or two minutes with alcohol; the iron solution is filtered upon it, allowed to remain for a couple of minutes, and then poured off. The pyrogallic acid solution is then filtered in a similar way upon it, and in the course of a minute or two—the desired depth of staining having been obtained—the tissue is washed, and may be mounted in the usual manner, in glycerin, balsam, or varnish. The nuclei and nucleoli will be found coloured black, and the cell substance will also be coloured more or less, according to the age and other conditions of the cell. A bluish tint may be given to them by washing the section with an alkaline water. The whole process is speedy, simple, and permanent. The staining may be accomplished in five minutes, and the materials can be bought at any country druggist's shop.

Dr. J. J. Woodward, of the Medical Department, United States Army, has made use of photography for micrometry where extreme delicacy is needed, as in the measurement of blood corpuscles for medico-legal cases. A glass plate is ruled in hundredths, thousandths, and five-thousandths of an inch: upon this a thin film of the blood to be examined is spread; this is best done by means of the edge of a glass slide. If dried blood-stains are to be examined, as in criminal cases, the fragments are soaked out by the usual processes. The photograph is taken, and the ruled lines appear with the image of the blood-corpuscles: the ordinary stage micrometer, covered with thin glass, cannot be employed, as the lines would not be in focus with the object. The measurements are made on the negative, under a magnifying glass, by means of a transparent scale ruled in hundredths of an inch on a thin strip of horn. To avoid parallax, the ruled side of the scale is placed in contact with the varnished film of the negative. The process is far superior in accuracy to the results obtained by any form of eye-piece micrometer, and to those possessing the necessary photographic appliances occupies little or no more time.

In the "American Journal of Microscopy" the Editor, Prof. Phin, has made some severe strictures upon the statements made by Prof. P. B. Wilson, of Washington University, Baltimore, Md., concerning the value of infusorial earth as an ingredient of fertilisers for cereal crops. In the October No. of this journal was an article, by Mr. W. H. Wahl, entitled "Infusorial Earth and its Industrial Uses," in the course of which incidental allusion was made to Prof. Wilson's alleged discovery: this was in substance that, having examined the straw from wheat grown upon land fertilised with a manure of which infusorial earth was an ingredient, he found, upon placing under the microscope what remained of the straw after it had been treated with nitric acid, that it consisted wholly of the siliceous shields of Diatomaceæ. Prof. Wilson alleged, in other words, that these diatoms, when incorporated with the soil, are taken up by the plant *as such*, without undergoing previous solution, and to prove his assertion he published an engraving illustrating quite a number of organic forms that he had detected. Upon the strength of these alleged observations Prof. Wilson advanced certain conclusions greatly at variance with current scientific opinion, which were stated in the article in question. While not sharing the opinions as to the significance of the presence of these organic forms in (Kunkel) straw, and expressing doubts upon the accuracy of his inferences, the writer did not presume to entertain a doubt as to the *genuineness* of Prof. Wilson's asserted observations. "The American Journal of Science and Arts" also admitted into its pages an elaborate article by Prof. Wilson, bearing the title "Silica of Grasses and other Plants carried up as Diatoms or other Siliceous Grains, and not in Solution or as Soluble Silicates." Prof. Phin emphatically denies the genuineness of Prof. Wilson's alleged observations. The following extracts from Prof. Phin's article are sufficiently explicit upon this point:—A single glance at the engraving to which he so confidently refers is sufficient to convince any microscopist that Prof. P. B. Wilson never saw 'upon the field of his microscope,' under the circumstances which he has described, the objects which he has delineated. This is a bold assertion and a severe accusation, but the proof is simple and unimpeachable." Prof. Phin then proceeds to establish his accusation by showing that one of the forms (*Bacillaria*) figured on the plate is figured as it exists only in the living condition (the organic matter, it will be remembered, has been destroyed by treatment of the straw with nitric acid), the frustules being joined together in the peculiar way which has given to this form the specific name *paradoxa*. "For this diatom," says Prof. Phin, "to have passed through a bath of nitric acid, and come out in the condition figured, would have been almost as great a miracle as passing Shadrach, Meshach, and Abednego unscathed through the fiery furnace of Nebuchadnezzar." He (Prof. Phin) affirms also that a *calcareous* Foraminifer is figured among the forms on Prof. Wilson's plate. If this be the case, we should require another miracle to have saved from destruction in the nitric acid this particular object; for it is a fact of common knowledge that carbonate of lime is energetically dissolved by nitric acid, even in the cold. Finally, Prof. Phin appends to his own criticism the statement of "one of our ablest diatomists," who declares that the thirty-six forms which Prof. Wilson has "carefully sketched" are composed of a *mixture* of marine and fresh-water diatoms, sponge spicules, a little of the siliceous cuticle of straw, and a Foraminifer. "*Only one form*," he remarks, "*belongs to the Virginia deposit with which Kunkel's field was fertilised—and which is exclusively marine.*" We hope that Professor Wilson will put us in a position to give his explanation in our next issue.

ENGINEERING.

In a pamphlet entitled "The River Clyde, an Historical Description of the Rise and Progress of the Harbour of Glasgow, and of the Improvement of the River from Glasgow to Port Glasgow," Mr. James Deas, M. Inst. C.E., gives an account of the successive improvements of the River Clyde, which in its present state may be deemed "as much an artificial navigation as the Suez Canal." Few, indeed, of the multitudes of strangers who annually visit Glasgow, and see the forest of masts in its harbour, would imagine that "so recently as one hundred years ago the river was almost in a state of Nature (!) and was fordable on foot at Dumbuck Ford, more than 12 miles below Glasgow." Unfortunately, whilst the Clyde has been widened and deepened, it has been polluted *pari passû*, and most certainly cannot now be accused of being in "a state of Nature." The author admits "The rights of salmon-fishing in the river were carefully protected in the earlier Acts of Parliament, and fishing-stations were numerous and of much value,—one station, with its hut, being within the precincts of the harbour,—but the whirr of the paddle, the churn of the screw, and, above all, the sewage from the vast population in the area which the river drains, together with the deleterious liquid refuse from numerous manufactories of all kinds, have driven away that much-prized fish." It is very true that the preservation of fish is not the highest national concern. We may import, or in case of need even dispense with salmon, but unfortunately we cannot forego our manufactories. Still there is no occasion that the pollution of rivers shall be accepted as a necessity. It may not be possible to restore the Clyde to its ancient crystalline purity, but its present condition is capable of vast improvement. It is very possible that the outlay for dredging the bed of the river might be notably reduced were the suspended impurities not washed down by the sewers duly arrested. The improvements in the navigation of the Clyde have not been effected without a very serious outlay. The total expenditure of the Trustees since 1770 amounts to £5,594,981, of which £1,390,947, or nearly one-fourth, has been spent in interest on loans. The present debt of the Trust (June 30th, 1872) amounts to £2,151,557. The expenditure for dredging alone has been upwards of £500,000, and during the last twenty-eight years 14,609,454 cubic yards of material, or upwards of 18,000,000 tons, have been dredged and removed. One result of the improvements carried out on the river since 1758 has been the gradual lowering of the level of low water in Glasgow Harbour to the amount of 8 feet, and even since 1853 it has fallen at least 13 inches. The author ascribes this change to the great extent to which deepening, widening, and straightening have been recently carried on in the lower reaches of the river, thus enabling the ebb tides to hurry out to sea more rapidly than heretofore. On the other hand, the high-water level of spring tides is now about 10 inches higher than in 1853. One evil which has been completely overcome is the liability of certain low-lying portions of the city of Glasgow to inundations. Since 1856 the river has not been over the harbour-quays.

The blowing-up of the reef at Hallett's Point, which formed such a dangerous obstruction in the Hell Gate Channel, and greatly hindered navigation through Long Island Sound, has been truly termed one of the most successful engineering exploits of the century. We are indebted to the "Popular Science Monthly" for the following concise account of the experiment. The reef was of an irregular crescent shape, some 700 feet long, and extending out 300 feet into the channel, with an area of about three acres. The rock is a tough hornblende gneiss, with veins of pure quartz, and lies in strata of various degrees of inclination. The plan of operations was to build a coffer-dam on the rock near the shore to bar out the water, to sink a shaft to the requisite depth, to honeycomb the whole rocky mass by excavation, and then to blow up the shell by charges of dynamite in the roof and supporting columns, to be fired by the agency of galvanic batteries. The shaft was sunk to a depth of 33 feet below the line of low water, and ten tunnels were then opened to distances varying from 31 to 126 feet. The cubic contents of the rocky mass above the depth of 26 feet at mean low water, amounted to 51,000 yards. The tunnels radiating from the shaft varied from 7 to 22 feet in height, and from 9 to 12 feet in width, and as they advanced the height rapidly decreased, owing to the downward slope of the surface of the reef. As the main tunnels diverged from each other, subsidiary tunnels were introduced, and a system of transverse galleries was excavated, and which left 172 supporting pillars of variable dimensions. The total length of tunnels was 4857 feet, and the length of galleries 2568 feet, making the entire length of passage excavated 7425 feet. The excavations being completed, so that the roof of rock above was reduced to a thickness of from 8 to 16 feet, the preparation for the explosion began by drilling the rock for the charges. The whole number of blast holes drilled into the roof and piers was 4427, varying from 7 to 10 feet in depth, and from 2 to 3 inches in diameter. Each one of these holes was charged with three kinds of explosives, all composed of nitro-glycerine, viz., dynamite rendrock and vulcan-powder in separate cartridges or canisters. Fifty thousand pounds of these explosives were buried in the apertures. Ninety-six galvanic batteries, of ten cells each, were employed to ignite the charges. The firing point was 650 yards from the shaft, and the amount of leading and connecting wire used to bring all the charges into relation with the batteries was 220,000 feet. The charges in the different holes of the same pier were connected so as to explode simultaneously, but a fuse composed of a quick explosive was used to connect the system of charges in each pier with those of the neighbouring piers. In this way the electric spark taking effect in a few centres, the ignition was propagated through the whole system, as the explosion of the connecting fuse would advance more rapidly than the destruction of the rock. The several thousand charges in the mine were connected in 23 groups of batteries. These were ingeniously connected in a mechanical arrangement so simple and perfect that a child could operate it, and the whole stupendous force that slumbered in the charges was actually released by the touch of a little daughter of General Newton, two years and a half old. The explosion was accompanied by no very stunning effects to eye or ear, and the demonstration was so moderate as to produce great disappointment in the crowds who assembled to witness it. There was a succession of shocks, lasting a few seconds, with no great noise, a mass of water and *débris* of the coffer-dam thrown into the air, and the great reef was shattered and demolished.

TECHNOLOGY.

At the meeting of the Chemical Society on November 2nd, a preliminary notice by Messrs. W. R. Hodgkinson and H. C. Sorby was read, on "*Pigmentum nigrum*, the Black Colouring-matter contained in Hair and Feathers." When perfectly white hair or feathers are heated gently with dilute sulphuric acid for some time they completely dissolve, but if black or brown feathers or hair are thus treated an amorphous black residue is obtained. This substance, which exists only in very small quantity in the blackest feathers, may be conveniently prepared from rooks' feathers (which yield about one per cent) which have been separated from the central rib, and thoroughly cleaned from waxy and fatty matter by treatment with alcoholic ammonia. On digesting them with successive quantities of dilute sulphuric acid for several days, until the acid ceases to be coloured by red or brown soluble colouring-matters, a black residue is obtained, which, after being thoroughly washed with dilute hydrochloric acid at 80° C., and then with water, is dried, and the last trace of fatty matter finally removed by treatment with boiling alcohol and ether. On analysis it gives numbers agreeing very well with the formula $C_{18}H_{16}N_2O_8$. It is not acted on by dilute acids or alkalies, but nitric acid slowly oxidises it. It forms new compounds by the action of bromine, one of which is soluble in water, and gives a characteristic absorption spectrum.

At the Chemical Works at Aalborg, in Jutland, Denmark, where about 30 tons of alkali are made per week by the ammonia process for obtaining alkali from seaweed, Mr. Thowald Schmidt, the Director of the Manufactory, proposes to work, in conjunction with this process, a method devised by himself of treating seaweed so as to obtain iodine, potash salts, and other marketable products therefrom. In Denmark a very heavy duty is levied on the importation of common salt, whilst enormous quantities of seaweed rich in iodine and potash can be obtained at small cost in the neighbourhood of the works. Mr. Schmidt's process is as follows:—After the seaweed is dried and burnt a concentrated solution of the ash is made and added to the liquor containing chlorides of sodium and calcium, left after the ammonia has been recovered in the ammonia-soda process by boiling with lime. The sulphates of potash, soda, and magnesia contained in the ash of the seaweed are thereby decomposed, and hydrated sulphate of lime and hydrated magnesia are precipitated in a form which may be available for paper-making as "pearl-hardening." The last traces of sulphates are got rid of by adding a small quantity of solution of chloride of barium. To the clear solution nitrate of lead is now added until all the iodine is precipitated as iodide of lead, which is then separated by filtration and treated for the production of iodine or iodides. After filtration the liquid is boiled, nitrate of soda is added to convert the chloride of potassium present into nitrate of potash. The latter is separated by crystallisation. There remains a solution of common salt containing traces of ammonia from the previous soda operation and a trace of chloride of potassium. This solution is again treated by the ordinary ammonia-soda process for the production of bicarbonate of soda and white alkali.

Under the title of "Country Laboratory Apparatus," Mr. Edward T. Hardman, F.C.S., of H.M. Geological Survey, Ireland, describes a substitute for crucible jackets, useful to those who have occasion to shift their quarters often, and are obliged to work with a necessarily limited laboratory accommodation. The ordinary crucible jacket being made of sheet-iron has in reality but one use—to protect the flame from currents of air. An ordinary earthenware flower-pot answers the purpose in every respect. It is the proper shape, and being made of a non-conducting material it in a great measure prevents loss of heat from the burner. The bottom of the flower-pot has a circular hole: this serves for the introduction of the Bunsen burner. As the supply of air would be otherwise insufficient, it will be necessary to enlarge the opening. This can be easily done by cutting the aperture nearly in the form of a cross, and not too large. A current of air is then obtained which not only steadies the flame, but acts in some degree as a blast. The flower-pot may be supported in the ring of a retort-stand in the usual way. The chimney is a second flower-pot inverted. To support it the handiest way will be to make three S hooks of stout wire, and having passed the narrow end of the pot upward through the ring, fix the rim within the hooks caught on the ring, as in Fig. 1. The apparatus acts admirably as a small gas furnace for crucible operations, such as the fusion of silicates with carbonate of soda—as in the analyses of rocks; while for simple ignition of precipitates it renders the flame of a common glass spirit-lamp most effective. The size of the flower-pot required will, of course, depend on that of the crucible and of the burner used. The support for the crucible may be either a triangle of wire covered with

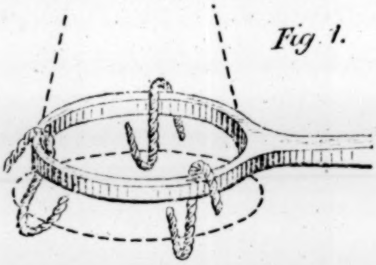
Fig. 1.

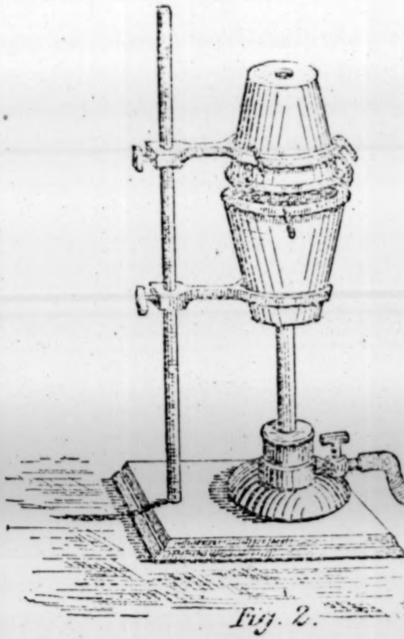
Fig. 2.

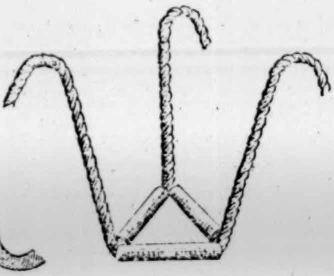
Fig 3

pipe-shank, the end of the wire being bent upwards and formed into hooks so as to hang on the edge of the flower-pot (Fig. 3), or three pipe-covered wires, suspended in the position of the ribs of a crucible jacket. The former is necessary for small crucibles. The flower-pot also makes an excellent lamp-screen, for steadying and concentrating the flame under evaporating basins, &c. A small flower-pot with wire gauze tied over the top is a very effective low temperature lamp when the gas is lighted before the gauze. If the gas is lighted above the gauze an argand lamp giving a large clear blue flame is obtained. In the latter case a common burner can be used. Having occasion to determine the volatile matter of a coal, and not having at hand the usual elaborate arrangements, Mr. Hardman thought of the schoolboy's method of manufacturing coal-gas. The retort he uses is a common clay tobacco-pipe. A piece of coal is put in, the top is luted with clay, and the pipe is inserted in the fire-grate with the stem projecting. Presently a dense smoke issues from it, and and a match being applied a veritable gas light—but not "16-candle" —results. On opening the luting a piece of coke is found in the pipe. It is obvious that it is only necessary to weigh the pipe and contents before and after the operation, and we have the volatile matter and coke determined. The larger the pipe the better. The coal must be broken small, but not powdered. The pipe is weighed, then filled with the coal and weighed again to obtain weight of coal. Then inside the top is fitted a circular piece of writing-paper, the use of which is to prevent any of the luting getting down among the coal, where it could not be removed, and would falsify the last weighing. The top is luted with moist fireclay, or with the cement used for luting the covers of gas retorts, and the pipe being placed in a common coal fire or in a gas furnace from ten to twenty minutes completes the operation. When cool the luting is carefully taken off and the charred paper removed. The pipe and contents being then weighed, the loss gives the volatile matter, the same weighing, of course, determining the coke. If a very exact determination is required, a quantity of the coal may be broken small, well mixed together, and four pipes filled as above. They can all be ignited together in a fire, and weighed very quickly. The results will be found to agree very closely. This tobacco-pipe process has the great advantage of being very expeditiously performed—the whole experiment including weighings not occupying more than thirty minutes—and with very simple apparatus.